SCIENTIFIC AMERICAN SUPPLEMENT. No 1785

Entered at the Post Office of New York, N. Y., as Second Class Matter.

Published weekly by Munn & Co., Inc., at 361 Broadway, New York.
Charles Allen Munn, President, 361 Broadway, New York.
Frederick Converse Beach, Sec'y and Treas., 361 Broadway, New York.

Scientific American, established 1845.
Scientific American Supplement, Vol. LXIX., No. 1785.

NEW YORK, MARCH 19, 1910.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.

INSTRUMENTS AND METHODS OF THE OCEAN MAGNETIC WORK OF THE CARNEGIE INSTITUTION OF WASHINGTON.

By L. A. Bauer, Director of the Department of Research in Terrestrial Magnetism.

THE GENERAL PRINCIPLES FOLLOWED.

From the very start of our ocean magnetic working on the chartered brigantine "Galilee," in 1905, two principles have been steadfastly held in view:

a. To get useful work done and make the results promptly known.

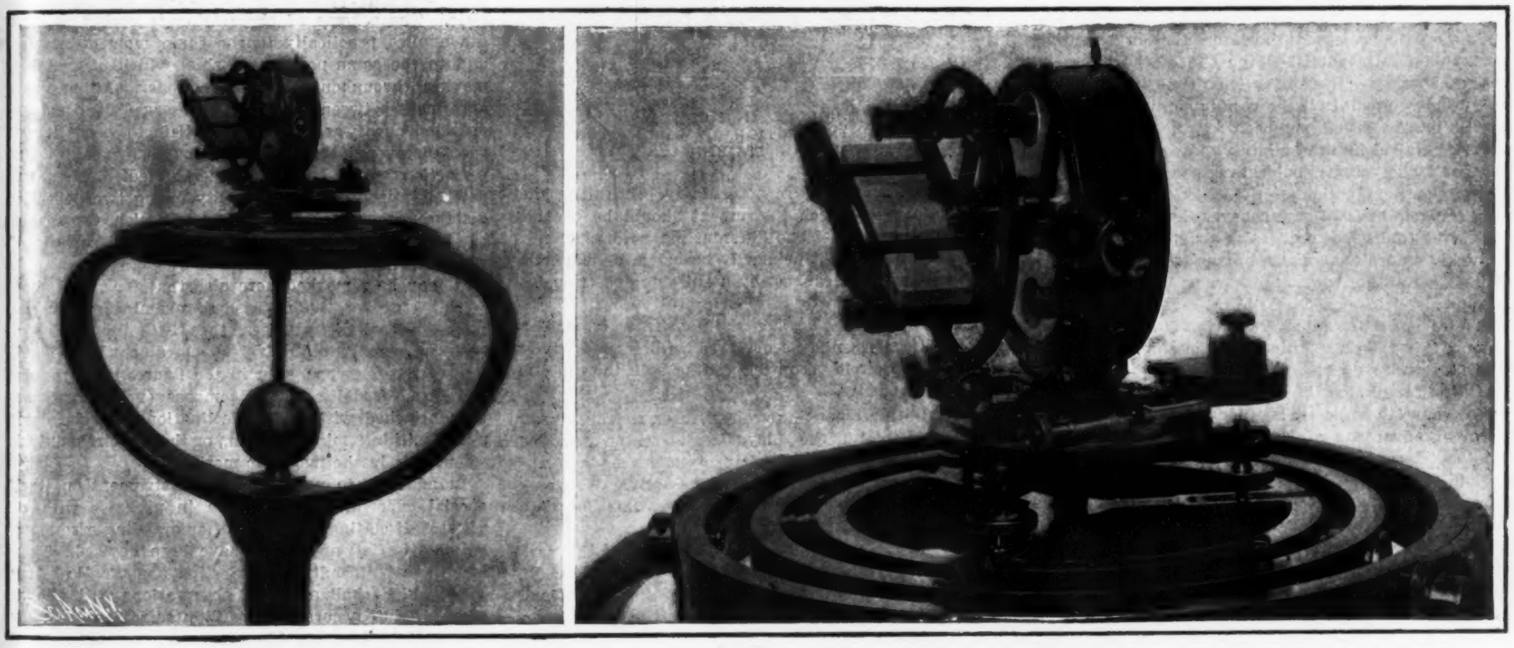
b. To strive toward the highest accuracy attainable in all elements, consistent with *a.*

Early in 1905 I spent a month abroad consulting various eminent investigators as to the requirements of ocean work, but could get practically no additional information to that which I had already obtained in my previous experience on Coast and Geodetic Survey vessels, magnetic work on board of which was begun under my direction in 1903.

It was concluded that the best procedure would be to make a beginning in the Pacific Ocean at once by selecting a suitable vessel to be chartered for a term of years. As the result of some advertising, the "Galilee" was finally chosen and as much as possible of the iron on board removed or replaced by non-magnetic materials. Next a special observing bridge* was constructed, on which the instruments were mounted. The result was that we had a vessel for which, at the places where the various magnetic instruments were placed, the deviation coefficients were the smallest of any ship thus far employed in magnetic work, inclusive of those specially constructed for Antarctic exploration, the "Discovery" and the "Gauss."

We next strove toward a symmetrical development of all the instruments. It was recognized as a mistake to pick out any one, e. g., an intensity instrument, and devote exclusive attention to it and disregard how it would fit in with the other appliances. Hence equal attention was bestowed from the start to all three elements, magnetic declination, dip, and the intensity of the magnetic force, every known method and instrument being studied and thoroughly tried out under actual sea conditions. Because of my previous experience, it was possible during the period of our trial trip from San Francisco to San Diego, in August, 1905, to settle on the methods which applied practically

* For an account of the work of the "Galilee" see Scientific American, February 15th, 1908.



THE L. C. DIP CIRCLE, MOUNTED ON GIMBAL STAND, AS USED ON BOARD THE "GALILEE" AND THE "CARNEGIE," SHOWING THE MODIFICATIONS MADE BY THE CARNEGIE INSTITUTION.

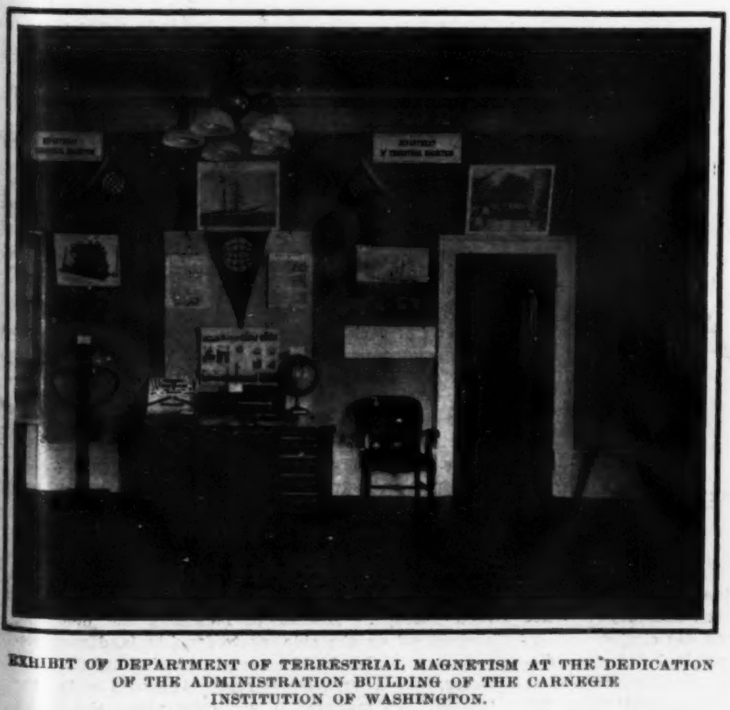
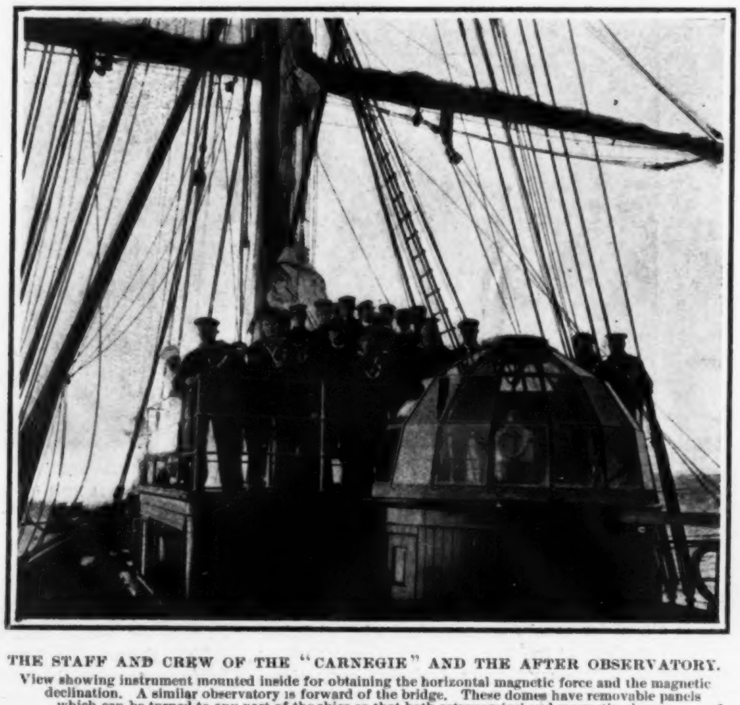


EXHIBIT OF DEPARTMENT OF TERRESTRIAL MAGNETISM AT THE DEDICATION OF THE ADMINISTRATION BUILDING OF THE CARNEGIE INSTITUTION OF WASHINGTON.



THE STAFF AND CREW OF THE "CARNEGIE" AND THE AFTER OBSERVATORY.

View showing instrument mounted inside for obtaining the horizontal magnetic force and the magnetic declination. A similar observatory is forward of the bridge. These domes have removable panels which can be turned to any part of the skies so that both astronomical and magnetic observations can be made under shelter.

A MAGNETIC CHART OF THE WORLD.

throughout the three years, 1905-08, the "Galilee" was in commission.

It was quickly shown that by the methods employed whereby each element was determined in two different ways with different instruments and by different observers, the observational errors were not only considerably less than the chart errors but were also, in general, less than or about on the order of the error of the ship deviation corrections. In other words the uncertainty of the deviation correction very soon became our chief concern. And if this was true with the precautions taken on the "Galilee" having smaller deviation coefficients than any other vessel engaged in magnetic work, viz., swinging ship every third or fourth day, then how much more must it be true on a vessel having larger deviation coefficients and less opportunity for swings, as was the case with the recent Antarctic vessels?

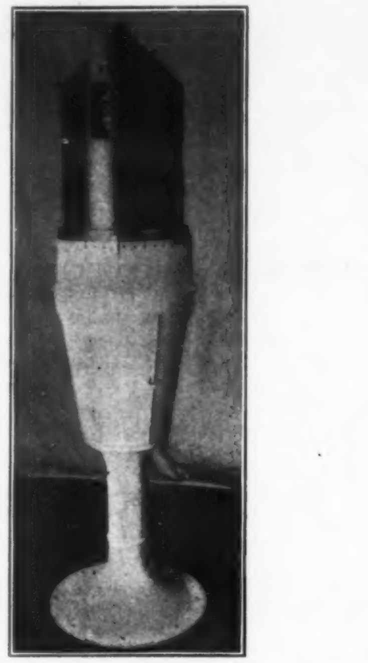
We saw no need, therefore, for deferring the getting of useful results until ocean instruments had reached the same state of perfection as land instruments, unless we were assured that funds would soon be available for the building of a wholly non-magnetic ship. This assurance we did not have when the work was begun on the "Galilee." In fact it was necessary to win confidence in our ability "to make good" what had been promised. And so we were determined to make the ocean work a success: instead of spending three years or more in elaborate office investigations and writing memoirs it was thought best to start right out and get some work done and, above all, make the results known promptly.

In June, 1906, it was already possible to call attention to large systematic errors in the magnetic charts of the three elements for the Pacific Ocean. Early in the spring of 1907 all the data obtained were supplied to the United States Hydrographic Office on the understanding that new magnetic charts of the three elements were to be issued; however, that organization has thus far only published the one of the "lines of equal magnetic declination." Some time in 1910 is to appear the volume of all the results obtained on board the "Galilee." Had it not been for the large amount of work entailed by the building and equipping of the new vessel, the volume could have been issued in 1909.

The prompt reduction of the observations and the many controls insisted on whenever the vessel reached port served to disclose the weak points, but not always as quickly as desired. Thus, because the deviation coefficients were different at the various positions of the instruments, it was not possible to get an immediate comparison, for example, in magnetic declinations made at two different stations on the ship. The deviation corrections could not be successfully determined until the completion of a cruise covering a large enough range in magnetic latitude. And here is where the great advantage of having a non-magnetic ship, like the "Carnegie," counts most heavily. It is now possible to make on board nearly a final computation a few minutes after completion of the observations and thus "check up" at once and repeat, if necessary.

THE INSTRUMENTS.

The available space on board ship for magnetic work is, necessarily, restricted and, in fact, is never as large as one would like—not even on the "Carnegie." Hence it becomes essential to arrange the instruments so that what is aimed for can be accomplished without bringing them so close as to have an effect on one another and thus introduce once more deviation corrections. There are three magnetic elements to be determined: declination, dip, and intensity. My general experience in magnetic work has forcibly impressed upon me the need of getting a totally independent check on each element of observation. Hence each is to be determined twice, simultaneously preferably, and so there either must be 3 by 2 or six different instruments or each instrument must be arranged to measure more than one element. The first suggestion is rarely practical because of the limited observing space and the desirability of taking advantage of the best possible conditions regarding steadiness of ship, etc.



THE "MARINE COLLIMATING COMPASS" OF THE CARNEGIE INSTITUTION OF WASHINGTON MOUNTED ON ITS BINNACLE.

Our developments have accordingly been along the second line: that each instrument should be capable of measuring at least two different magnetic elements.

Thus an instrument primarily intended for magnetic declinations was arranged, by a suitable deflection device, to measure also the horizontal intensity; one arranged chiefly for horizontal intensity was made so that declination could be observed with it, and finally the L. C. dip circle permitted getting both the dip and the total intensity.

THE SHIP DIP CIRCLE AND TOTAL FORCE INSTRUMENT.

Beginning with the Lloyd-Creak or the so-called "L. C. Dip Circle" the chief improvements made on the original instrument as devised by Capt. E. W. Creak and as supplied to the Antarctic expeditions of 1901-04 were as follows:

Insistence on perfection in construction of the various parts of the instruments by the maker, A. W. Dover, of Charlton, Kent, notably of the pivots and jewels. We finally succeeded, due to Dover's skill, in getting instruments for which the accuracy was not so very far behind that with land dip circles.

Next it was found that the original deflection distance was a trifle short and, in consequence, deflections were impossible when the earth's magnetic force fell below a certain value. Thus on the 1905 cruise the instrument became unavailable for magnetic intensity observations before the vessel reached Honolulu. A similar experience was encountered by the Coast and Geodetic Survey steamer "Bache" on a trip to Jamaica and Colon. Accordingly the distance was sufficiently increased and at the same time a second deflection distance introduced, making the instrument now everywhere available—something which the horizontal intensity instrument is not. The latter is not suitable, for example, in regions of high magnetic latitudes, where, in consequence, the force acting on a compass needle would be weak. A brass holder for the deflecting needle was made so as to avoid handling the needle during a set of observations, the passage from short distance to long distance being accomplished by a simple inversion of the holder, the needle being mounted eccentrically inside.

Next the milled heads of the footscrews were graduated and means provided for insuring that the instrument when mounted on the gimbal stand was actually level. The heights of the footscrews were repeatedly determined and controlled for an invariable and level position of the circle, whenever the sea was calm or when the vessel was in port, and thus the instrument set level in between. I do not remember seeing described in any book on ocean magnetic work how the dip circle was actually set level on the gimbal stand, although the full error of level may go into the dip. (An accidental missetting of the footscrews upon one occasion, by less than one complete turn, produced an error of about one degree and a half in the dip.)

The method of observation invariably followed yields four determinations of dip, two of these being with the regular dip needles according to the absolute method, inclusive of reversal of polarity of needle, and two being "deflected dips," i. e., those resulting from the deflection observations at two distances for getting total intensity, hence not involving any additional time. The scheme of observation is such that each dip applies practically to the same moment of time and to the same position of ship, which of course is moving throughout the observations. At first the agreement between the directly observed dips and the deflected ones was not always satisfactory, but it was soon discovered that this was due to mechanical imperfections which have since been remedied. Next, two determinations of total intensity, using two deflection distances, are obtained. Should one wish, the scheme can readily be extended to include different sets of needles, etc.

It may be remarked here also that for rapid shore work, e. g., for the purpose of examining into pronounced local disturbances, the L. C. dip circle has been converted into a universal instrument by the addition of the telescope, seen in the figures, thus making it into a theodolite, and a compass (not shown) has been attached with which declinations may be determined within 2 min. or 3 min., or the dip circle quickly set with it in the magnetic meridian.

In addition to perfecting the instrument in itself, special experiments have been in progress with the view of disclosing the cause of outstanding errors, and suitable testing appliances have been devised. There is no great difficulty in perfecting a magnetic instrument which shall admit of observations with the desired absolute accuracy over a limited region, but when it comes to insuring the same accuracy over practically the entire globe, then problems present themselves scarcely to be appreciated. Even for land instruments the problem is not such a simple one, as has been repeatedly found. Hence it is of the utmost importance to get independent checks in every possible manner. Every opportunity has been embraced in port to get shore intercomparisons between all ship instruments and land outfits (our own as also those of local observatories). With these improvements and the precautions taken, an accuracy sufficient for all purposes is being obtained by us with the improved instruments. Some further improvements are in progress.

SHIP HORIZONTAL FORCE INSTRUMENT.

Early in 1905, on account of the failures met with the L. C. dip circle in getting total intensities in low magnetic latitudes, as above reported, I undertook the devising of a special deflecting apparatus for horizontal intensity observations, used throughout the "Galilee" work, which could readily be attached to the ordinary navigation compasses. At that time the Bidlingmaier "double compass" had not been perfected, and even if it had it would not have answered our requirements. We desired the simplest possible contrivance both from an instrumental standpoint and that of computation.

Accordingly there was developed a deflection arrangement based on the sine-deflection formula, which implies that the deflecting magnet is at right angles to the deflected one when the state of equilibrium has been reached. Instead of mounting the deflecting magnet off on the side, as is done in land magnetometers and also in Neumayer's "deviations magnetometer," used on the German Antarctic ship, the "Gauss," we mounted it vertically above the center of suspension of the compass card.

The magnets used in deflecting the compass card,

SHIP HORIZONTAL FORCE INSTRUMENT MOUNTED ON TRIPOD FOR SHORE OBSERVATIONS AT THE HONOLULU MAGNETIC OBSERVATORY, SEPTEMBER, 1907.

A MAGNETIC CHART OF THE WORLD.

the angle of deflection being a measure of the magnetic force, were especially selected ones which could be quickly attached in a rigid manner to the deflector framework for horizontal intensity work and again readily removed for magnetic declination observations. In the Bidlingmaier or German instrument both the deflecting and deflected magnet systems are movable compass cards, and the instrument, for best success, ought to be treated entirely as an instrument by itself. In our device the same instrument used for navigation of vessel, or for getting magnetic declination, can be converted in an instant to a horizontal intensity instrument and the design of not multiplying instruments unduly may be carried out.

The German instrument possesses apparently, however, one advantage over ours, viz., with it one is independent of the ship's head or constancy of direction of lubber line, since the angle made by the axes of the two compasses is at once twice the deflection angle which measures the magnetic force. In our device it is necessary to read the deflected positions of compass from some zero or index—as the lubber line—which moves as the ship turns. To overcome this difficulty two methods have been followed leading practically to identical results:

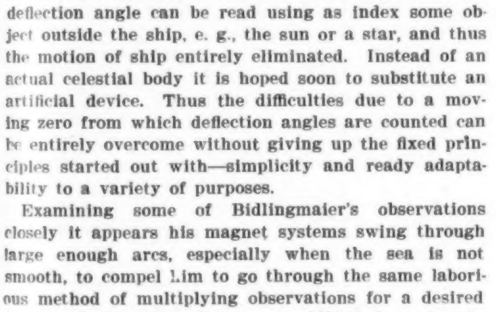
The helmsman is ordered to hold a given course to the best of his ability for about an hour, during which numerous deflection readings are made in four positions of magnet and circle. It is remarkable how at the end of a half to a whole hour the effect of yawing of a ship is eliminated, the yawing being on the average as much to port as to starboard. Hence, while individual deflection sets may be quite erroneous, the average yields a result comparing favorably with that derived from the dip and the total intensity observed with the L. C. dip circle.

A method which has been more favorably employed is to take simultaneous readings of the ship's head with the steering or other spare compass close by and then apply the necessary corrections to the observed deflection angles. Thus the agreement in the individual sets has been improved though the final result is practically the same as by the first method.

Furthermore, on the "marine collimator compass" aboard the "Carnegie," it has been arranged that the deflection angle can be read using as index some object outside the ship, e. g., the sun or a star, and thus the motion of ship entirely eliminated. Instead of an actual celestial body it is hoped soon to substitute an artificial device. Thus the difficulties due to a moving zero from which deflection angles are counted can be entirely overcome without giving up the fixed principles started out with—simplicity and ready adaptability to a variety of purposes.

Examining some of Bidlingmaier's observations closely it appears his magnet systems swing through large enough arcs, especially when the sea is not smooth, to compel him to go through the same laborious method of multiplying observations for a desired degree of accuracy as we are obliged in order to eliminate the motion of the lubber line.

In the instrument on board the "Carnegie" (see fig-



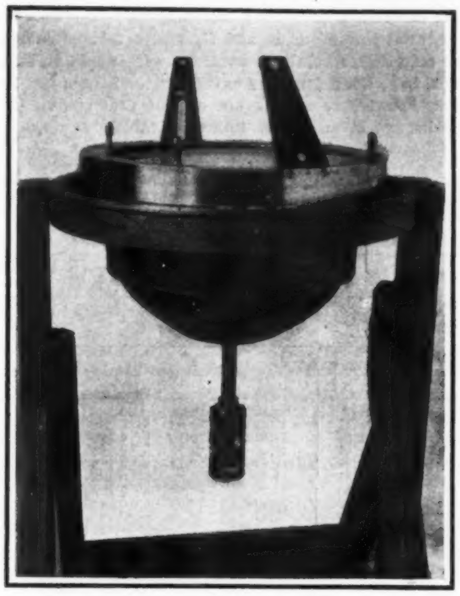
A SECTION THROUGH THE "MARINE COLLIMATING COMPASS."

View showing the four magnets R, R, R, R; two of the four collimating lenses N, N, attached to the buoyant ellipsoid A; the scales S, S, each placed in the focus of the lens N, to which it belongs; and G, G, are two of the four windows through which the scales are read. These windows are segments of a spherical shell whose center is at the point of support of the optical system, hence the rocking of the bowl or the rotation produced by yawing of the vessel does not alter the optical conditions.

ure above) the deflecting magnet is mounted below the card instead of above as was originally the case. The magnet may be easily placed at four invariable deflection distances. The construction has been improved in other respects by my assistants, Messrs. Fleming and Widmer.

INSTRUMENT FOR MAGNETIC DECLINATIONS.

While my attention was being devoted to perfection of appliances and methods for dip and intensity, my assistant, Mr. W. J. Peters, was making a special study of instruments for getting the magnetic declination at sea. Every known appliance was put to a practical test—they all were found subject more or less to the error arising from the inevitable motion of the compass card while the magnetic bearing of the celestial body was being taken. To eliminate this error it was necessary to extend the observations over a sufficiently long interval so that on the average it could be assumed that the various motions of the card had been eliminated. This had to be done more or less blindly, however, as one could never tell just at what point of the arc of motion the magnetic azimuth of the stellar body was obtained. Furthermore the various instruments all had movable parts subject to wear with frequent use, such as the axes of mirrors or of prisms and of the azimuth circle on the bowl. Likewise, graduation errors on the card had to be considered. Owing



VIEW OF NEW DEFLECTOR FOR HORIZONTAL FORCE OBSERVATIONS BEFORE BEING MOUNTED IN ITS BINNACLE ON THE "CARNEGIE."

to these defects there were introduced "apparent" deviation corrections not due to the ship's magnetism but to purely instrumental causes. To be able to separate the "apparent" from the "true" deviations it was necessary on the "Galilee" work to go through an elaborate series of shore observations whenever the vessel reached port. The ship instruments were invariably dismounted and used ashore alongside of the customary magnetic outfits for land work.

As a result of the investigations the "marine collimating compass," here described, was devised by Messrs. Peters and Fleming and constructed with great skill and conscientious care by Mr. Adolf Widmer, chief instrument maker of the Department of Terrestrial Magnetism. Such an instrument is now in successful operation on the "Carnegie." No such attention, as far as known, has been given by anyone else toward the perfection of compass work.

The basis of the instrument is a Ritchie United States navy standard 8-inch liquid compass with the card, however, removed, and an optical collimating system with scale introduced, enabling the observer to note the arc of motion of the magnet system while sighting on the sun or a star, hence knowing precisely to what part of the arc the stellar azimuth applies. In brief, practically the same method of observations can now be used at sea as on land, where the magnetometer circle would be set to some convenient point on the magnet scale and then scale readings taken of the positions of the magnet during the interval of observations. The angle is next determined between the circle setting and some mark, or the true meridian, and the magnetic declination is finally deduced. Similarly with the marine collimator compass. The angle (say, middle of scale) between the magnet and some celestial body, as the sun, is read with a pocket sextant to the nearest minute of arc at a given time, and then readings of the magnet scale and the watch are taken. With the aid of the time readings, the motion of the sun during the interval of observation is taken into account, and the true azimuths determined, whereas the scale readings give the varying positions of the magnet system.

With this instrument,* therefore, one is almost entirely independent of the yawing and rolling of the ship, making it possible still to get satisfactory results when with all other azimuth circle devices, hitherto used at sea, observations would be wholly impossible, or, at least, very uncertain.

In ten minutes a value of the magnetic declination may now be obtained possessing an accuracy attainable with previous instruments only by most careful observation and by laborious repetitions extending over a half hour or more to eliminate the motion of the card. Thus not only has the accuracy of declination at sea been increased, but, what is equally important, the time has been reduced and the possibility of getting useful results in all kinds of seas greatly extended.

It may also be pointed out that the effect of drag of magnet system moving in the liquid during changes of the ship's head is overcome in the present instrument, as well as in the deflector above described, for the method of observation involves turning the compass bowl opposite to the ship's motion.

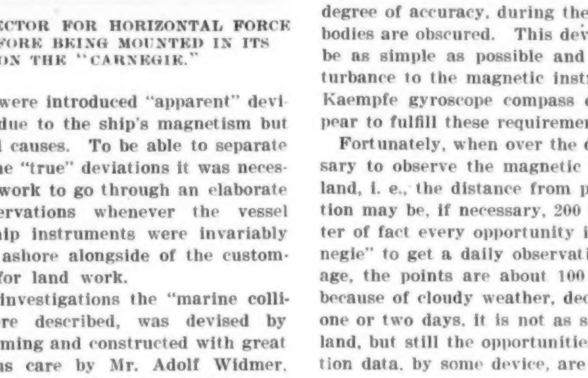
The chief outstanding difficulty, at present, is to get determinations of the magnetic declination when no celestial body is visible. In other words a device is desired which will furnish at sea an azimuth mark whose direction will be invariable, within the desired degree of accuracy, during the intervals when celestial bodies are obscured. This device for best success must be as simple as possible and not be a source of disturbance to the magnetic instruments. The Anschütz-Kaempfe gyroscope compass does not, at present, appear to fulfill these requirements.

Fortunately, when over the deep seas, it is not necessary to observe the magnetic elements as often as on land, i. e., the distance from point to point of observation may be, if necessary, 200 to 300 miles. As a matter of fact every opportunity is embraced on the "Carnegie" to get a daily observation so that, on the average, the points are about 100 to 150 miles apart. If, because of cloudy weather, declinations are missed for one or two days, it is not as serious as it would be on land, but still the opportunities of multiplying declination data, by some device, are exceedingly desirable.

MISCELLANEOUS CONSIDERATIONS.

Practical schemes for ocean observations to secure a desired result had to be devised and a number of printed forms prepared so as to simplify both the recording and the reducing of observations. But few hints could be gleaned from the reports of previous expeditions as to the best methods for determining the deviations, due to the ship itself, for all three elements. It remains to be seen whether any other expedition has gone into these matters as fully as was done for the "Galilee." Fortunately this troublesome part of the ship work has been eliminated on the "Carnegie."

It has been my observation that there is nothing so



Kelvin compass. Horizontal force instrument. Ritchie compass. Dip circle and total force instrument.

Scientific Personnel.

J. P. Ault, Magnetic Observer. H. E. Martyn, Surgeon and Recorder. J. C. Peters, Commander.

OBSERVING BRIDGE OF THE "GALILEE" AND INSTRUMENTS.

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* For a more detailed description the reader may be referred to the Journal of Terrestrial Magnetism and Atmospheric Electricity for March, 1909, vol. xiv, p. [illegible].

harmful to magnetic research as lack of promptness in the reduction of the observational data and publication of the results. To accomplish prompt reduction it is necessary to begin right, viz., to have instruments, methods of observation and those of computation, all form one harmonious whole and not be treated as though they were independent of one another.

In a project such as the magnetic survey of the earth, one could readily lose himself and spin out the survey so interminably that it would take nearer fifty years than ten or fifteen years—the time allotted to complete it—with no added advantage whatever. On the contrary the application of proper secular variation corrections for a survey extending over fifty years would be a serious matter.

An observer ought never to forget that his observations have no value until they have been computed, and hence he must bear the computer in mind and consider what his task will be. It is astonishing how such a simple truth as this is at times disregarded. Having all these experiences in mind the work of the "Galilee" was arranged accordingly, and still further has it been possible to put into actual practice these ideas on the "Carnegie."

Two days after the completion of the observational work at Falmouth, England, a summary of the magnetic results obtained on the "Carnegie" from Long Island Sound to Falmouth (September 1st—October 18th), embracing the three elements, were sent to the chief hydrographic offices issuing magnetic charts, and important errors were pointed out. The summary was published in Nature ten days after the date of the last observation, and Gen. Rykatcheff presented the information before the Russian Geographic Society on October 27th. Some time in March of this year the complete results of the entire first cruise of the "Carnegie" are to be published. After leaving Falmouth, England, the vessel proceeded to Madeira, thence to Bermuda, and returned to Brooklyn early in February.

By the methods adopted it is possible for the director, in his office in Washington, to be almost as close in touch with the work as though he were continuously on the vessel himself. In brief, it may be said that the "Carnegie" is the most effective instrument of research in terrestrial magnetism thus far devised for mapping the earth's magnetic forces. With the present possible rate of progress on land as well as at sea, we may confidently expect the completion of a general magnetic survey of the greater part of the earth by 1915.

One of the first lessons necessary to learn in ocean work is, that it is rarely, if ever, possible to have ideal conditions. In consequence the development of good judgment of the observer is one of the prime requisites. Sometimes in an instant he must be able to change his plan of observation and be content with a fair degree of accuracy or get no observations at all.

Geographical Positions.—Equal care is bestowed upon the determination of the geographic positions of the points where the magnetic observations are made. The astronomical observations and computations are made in duplicate, and at times in triplicate, by the observers, and thus the positions are effectively checked. Six well-tested chronometers are carried aboard. With the methods followed, it would appear that the errors in the final positions assigned will, in general, be less than three minutes of arc in latitude and in longitude.

TESTS OF INSTRUMENTS AND OF VESSEL.

Both with the view of testing the various appliances on the "Carnegie," as well as to ascertain whether we had succeeded in getting non-magnetic conditions at the three places (*A*, *B*, and *C*) where the instruments are mounted, special observations were made at Gardiner's Bay, Long Island, August 31st to September 2nd, 1909, just before setting out on our first cruise. The results are given in the following table:

Summary of Swing Observations at Gardiner's Bay, Long Island, New York, August 31st to September 2nd, 1909.

Ship's Head.	West Declination.		Dip.	Horizontal Intensity in C. G. S. Units.	
	M.C.	Deflector.	North.	L. C. D. C.	Deflector.
	Position A.	Position C.	Position B.	Position B.	Position C.
	Degs.	Degs.	Degs.		
N.	11.41	11.41	72.02	0.1823	0.1830
N.E.	11.39	11.52	72.12	0.1823	0.1824
E.	11.45	11.48	72.11	0.1823	0.1830
S.E.	11.39	11.29	72.11	0.1826	0.1824
S.	11.34	11.41	72.09	0.1825	0.1831
S.W.	11.38	11.28	72.04	0.1824	0.1827
W.	11.32	11.40	72.08	0.1822	0.1823
N.W.	11.46	11.42	72.18	0.1823	0.1822
Mean	11.39	11.40	72.09	0.1824	0.1826

The vessel was "swung" with both helms on three successive days and magnetic observations were made on each heading of vessel. Looking over the results we notice not only a good accord among the instruments themselves, but also find the values of the various magnetic elements the same on each heading of ship within the errors of observation, showing the absence of deviation corrections of whatever kind. Only one who has had experience both in the observations and reductions of magnetic elements determined at sea can appreciate the full significance of these results and can realize the advance made.

Similarly the special observations made in Falmouth Bay after our trip across the Atlantic again showed the absence of corrections due to any possible ship's magnetism. It is chiefly due to this happy circumstance that the present ocean work is being not only expeditiously accomplished but also with the requisite accuracy.

Since the rapidity of the present work naturally will raise the question in the minds of some as to its thoroughness, I may say the work is open to the inspection of anyone at any time. A special point is being made to extend invitations to prominent scientific men to visit the "Carnegie" when in port, not only for the purpose of becoming acquainted with the instruments and the methods, but above all to give us the benefit of any criticisms or suggestions they may make. I am privileged to quote from letters addressed to the President of the Institution, Dr. R. S. Woodward, by two of our most eminent visitors at Falmouth:

"So far as I know, the 'Carnegie' is the first ship that has been especially built and designed for magnetic sea work, and I was very much struck not only with the care which has been exercised to avoid errors due to the presence of iron, but with the ingenuity with which the instruments have been arranged or modified in view of the special purpose for which they were intended. In particular I was pleased with the skill with which two instruments placed in different parts of the ship could be used to measure the same element, thus affording a valuable check on the accuracy of observations."—Sir Arthur Rücker, F.R.S., Newbury, England, November 6th, 1909.

"Last week I visited Falmouth in order to have a look at the 'Carnegie,' and I should like you to know how very much pleased I am with all the arrangements on board. . . . There cannot be the slightest doubt that the results which the 'Carnegie' will obtain will be of the highest importance not only in clearing up difficult problems of terrestrial magnetism, but also for practical purposes of navigation.—Prof. Arthur Schuster, F.R.S., Manchester, November 8th, 1909.

SOME OF THE RESULTS.

Already important errors in the magnetic charts at present in use have been found by the "Carnegie." These errors, owing to their systematic run for very long distances, are of sufficient moment to be taken into account in practical navigation. Thus it was found that the effect of the errors on the transatlantic tracks from New York to England is always to set the course of a vessel to the northward, whether she is coming from the east or the west. Off Cape Race, Newfoundland, a vessel may find herself too far to the northward of her track from 10 to 20 miles, dependent upon circumstances and upon how long she may have failed, because of possible cloudy conditions, to control her position by astronomical observations.

The observations made on board the "Carnegie" are accepted as correct by the leading establishments issuing magnetic charts.

POSITIVE COLORS.

THE following process, suggested by Prof. Namias, permits of the preparation of lantern slides in almost any color, and obviously it is also applicable to the production of the three constituent positives in the three-color process. The transparencies may be made on any plates; but they should not be too dense. After a careful washing, subsequent to fixing, they should be immersed in a mixture of equal parts of: (a) Potassium ferricyanide 1 ounce, distilled water 9 ounces; (b) lead acetate ½ ounce, glacial acetic acid 45 minims, distilled water 9 ounces.

The plate should be left in this mixture till the image is quite bleached—that is, for not less than ten minutes—then washed thoroughly till the gelatine shows not the slightest tinge of yellow. It should then be immersed in a 5 per cent solution of sulphuric acid, which converts the image of lead ferrocyanide into lead sulphate. This may be omitted; but better results are obtained thereby.

After another rinsing the transparency is immersed in the following: Dye 4½ grains, sodium acetate 85 grains, distilled water 20 ounces.

The number of dyes that can be used is very great; but they must be basic and not acid dyes. The following are a few that give good results: For three-color work—for red, fuchsine; for yellow, auramine; for blue, methylene blue. For ordinary purposes: Red, rhodamine B, xylene red; orange, acridine orange; yellow, auramine; green, methylene green; emerald green, brilliant green; blue, methylene blue, Victoria blue; violet, methyl or crystal violet. The images on bromide and gas-light papers may be converted into dye images in the same way; but there is considerable danger of the paper being stained. It is advisable to immerse the prints, after the lead bath, in a 1 per cent solution of nitric acid for about ten minutes, and then wash and immerse in a very dilute dye-bath—the weaker the better, as it then will not stain the paper, and, although the print may have to be left in for twenty-four hours, the image will absorb the whole of the dye. When the image is dyed deep enough, wash in repeated changes of water till the gealtine is no longer stained.

The image now contains silver ferrocyanide, and it is as well, though not essential, to remove this, which can be effected by the following bath: Hypo 2 ounces, water 16 ounces; dissolve, and add sodium acetate 1 ounce, glacial acetic acid 48 minims, water 4 ounces. Two or three minutes is sufficient to thoroughly remove the silver. The pictures may then be washed and dried; or, to make them more permanent, they can be immersed in alum 1 ounce, water 20 ounces, copper sulphate 44 grains, for two or three minutes, and again rinsed and dried. The colors obtained are very brilliant and very permanent.—The Photographic Monthly.

WEIGHING THE EARTH.

"WEIGHING the Earth" was the subject of a paper read by Prof. Poynting to the members of the Midland Institute Scientific Society at Birmingham. At the outset the professor remarked that in a strict sense the earth had no weight at all, because the weight was really the pull of the earth itself on any other body. And as the earth was pulling on all parts of itself equally in all directions, the net result was, of course, no pull at all. Yet they could imagine an experiment by which the earth might be weighed. If they had a large pair of scales fixed at some point of the earth's surface, and if they could only imagine to bring up the earth bit by bit, cubic foot by cubic foot, weigh it, and then return it to its place—if they could only get hold of the whole earth in this way, they could really weigh it. The experiments he was about to describe gave some idea of what the result would be. It would work out at 13,000,000,000,000,000,000,000,000 pounds—or the figures 13 with 24 noughts after them.

That was a clumsy result, and could be expressed in another way. The weight of a cubic foot of water was about 62½ pounds, and the average weight of a cubic foot of earth was about 350 pounds, or about 5½ times as much as the cubic foot of water. They could express the result by saying the density of the earth was 5½, meaning it was 5½ times the weight of an equal volume of water. But this was quite imaginary, for specimens of the earth could only be obtained by mere scratches upon its surface. They must look for some other method. They could show by means of the pendulum that the earth pull varied at different points on its surface. By timing the same pendulum at different parts of the surface, they could find how the weight varied from point to point. Peary was about one-third of a pound heavier at the North Pole than when he started on his great journey.

But there was something in a given piece of matter. It remained the same wherever it was taken, and that which remained the same was its mass. What was meant by the mass remaining the same was that the same effort would produce in it the same motion wherever that effort was made. Therefore, what they were really seeking, when they talked of weighing the earth, was its mass. The possibility of finding it was due to Newton, for he taught that bodies pulled each other with forces depending solely upon their mass and their distance apart. He showed the way, and ten years after his death the first real weighing experiment was made. Prof. Poynting described this, and later experiments carried out by Maskelyne and Hutton on Schiehallion, in Perthshire, which resulted in the density of the earth being determined at 4½. Then came the investigations by Mitchell and Cavendish, and finally those of Prof. Boys, who found the earth had 5.527 times the density of water. Describing in detail the experiments he had made, Prof. Poynting said they had their birth in Manchester, passed their early youth at Cambridge, and came to their full age in Birmingham. The final result he obtained was 5.493.

According to the Engineering Record, iron-slag block pavement has been used to some extent in the Borough of Richmond, New York, and has proved very satisfactory. The chief assistant engineer considers it one of the least noisy of the hard pavements, and capable of withstanding a great deal of wear. As an experiment, a row of slag headers was placed along the inside edge of car rails running through streets paved with asphalt blocks, where it is reported to have proved very effective in resisting the grinding wear of wagon wheels.

THE ARTIFICIAL SILK INDUSTRY.—II.*

CONVERTING WOOD INTO SILKEN FABRIC.

BY W. P. DREAPER, F.I.C.

Continued from Supplement No. 1784, Page 167.

PROCESSES OF MANUFACTURE.

Chardonnet Process.—The original plant at Besançon started with an output of 50 kilos. a day in 1891; it had reached 1,500 kilos. in 1904, and 1800 to 2.000 kilos. in 1907, and seems to remain at that figure in 1909. The breaking strain is given at 1.5 grm. per denier, and the elasticity at about 15 per cent. Whether elasticity is quite the term to use in the case of artificial silk is perhaps open to question. It is doubtful if the "stretching before breaking" which takes place can be compared with the actual elasticity of the real fiber.

Great precautions are necessary in the production of the solution of guncotton. The polariscope is used in determining the correct state of solution. The jets through which the collodion is squirted are accurately regulated by micrometric measurement. The process of denitration is, of course, a reducing one, and the details remain a secret. Temperature of the bath is a consideration, and the great aim is to reduce the loss of strength to a minimum. Years of study have greatly improved this operation, and have produced a thread which varies very little in this respect from day to day.

The statement is made that in 1907 2,500,000 liters of alcohol were consumed in the Besançon works, so that each kilo, required between 4 and 5 liters of alcohol in its manufacture. A kilo. of 199 denier thread of this silk contains 90,000 meters in length, or nearly 2 million meters of single filament as squirted from the jets. The selling price of this product has been given as 30 fr. per kilo, in 1896, 26.50 fr. in 1897, and 21.75 fr. in 1898; in 1903 it reached the price of 40 fr. per kilo., and it may be taken at 20 fr. to-day.

Zinc chloride process.—The first patented invention dealing with the production of artificial filaments by the direct solution of cellulose in aqueous solution and without the intervention of the nitrating process was made by Wynne and Powell in 1884 (Eng. Pat. 16,805 of 1884). These investigators seem to have confined their attention to the industrial production of electric light filaments, and this process has proved itself of great value in this direction and is in use to-day.

In conjunction with H. K. Tompkins, I took out further patents in 1897 dealing with details. In one of these patents the advantage obtained by "drying of the fibers or threads in a considerably stretched condition" was emphasized. This practice has since found a place in all the artificial silk processes dealing with the direct solution of cellulose which have since reached the industrial stage; and without it, it is impossible to obtain yarns of the maximum brilliancy.

Bronnert in 1899 patented the preliminary mercerizing of the cellulose and claimed that the solution was correspondingly facilitated. A good deal of work was done in this country by the Cellulose Silk Syndicate, Ltd., in connection with this process, and also by Bronnert on the Continent; but in spite of statements to the contrary, I do not think that this process has ever been worked on a very large scale.

The cupro-ammonium method.—As before mentioned, this originated with the Despeissis patent (Fr. Pat. 203,741, Feb. 12th, 1890). Nothing was, however, done with the process until Pauly re-patented the process in 1897 (Eng. Pat. 28,631 of 1897). The cellulose is dissolved in an ammoniacal solution of a copper salt. The details of this method are, of course, not made public, but after filtering a satisfactory solution may be obtained. The coagulating solution may be a 15 per cent. solution of sulphuric acid, copper sulphate and ammonium sulphate being produced, and the cellulose is precipitated in the thread form, and wound on suitable winders, which are usually made of glass. The newly-formed fiber is then washed on these holders with fresh water.

E. Bronnert (Eng. Pat. 18,884 of 1899) claims the treatment of cellulose with caustic soda, and then copper sulphate is added. When ammonia is added to the resulting mixture of cupric hydrate cellulose and sodium sulphate, a solution of the cellulose is obtained. Many patents have since been taken out, and it is obviously impossible to disclose the exact procedure in any works, even if they are known.

The recovery of the solvent materials is mentioned later. Fremery, and Urban have observed (Eng. Pat. 20,630 of 1989), that it is advisable to dry the product in two stages: First at 104 deg. F., and afterward at a higher temperature. If the yarn be submerged in water at a temperature of 158 deg. to 212 deg. F. they claim that a dehydrating action takes place with beneficial results.

Viscose Process.—In the year 1892, Cross, Bevan, and Beadle (Eng. Pat. 8,700 of 1892) patented their method of bringing cellulose into solution for industrial purposes, but it was not until 1903 that Stearn (Eng. Pat. 1,020 of 1898), disclosed a commercial method of preparing filaments by precipitating this solution in the required manner by means of a solution of ammonium salts. This process was found to offer special difficulties, but to-day they have been overcome; and as a result of the initial work and first experiments at Kew, works have been erected at Coventry by Messrs. Samuel Courtauld & Co., Ltd., where this product is being produced in increasing quantity. The process is also being worked at Sydowsaue, near Stettin, in Germany (Kunstseide und Acetatwerke Fürst Henckell Donnersmarck), in France, at Arc-la-Bataille, near Dieppe (Societe Francaise de la Viscose), and in Italy.

Further important work has been done in this country on this process by Courtauld and Wilson (Eng. Pat. No. 21,405 of 1907), who suggested the addition of glucose to the precipitating bath, and Topham (Eng. Pat. 23,158 of 1900) who applied the turbine method of collecting the threads to these artificial fibers, an extension, I believe, of its previous use in the spinning of very short-fibered yarns, such as asbestos. Many patents have been taken out by other investigators, which deal with the preparation of the solution, methods of squirting, and subsequent treatment of the yarn, but they are too numerous to mention here. It is interesting to note in passing that the jets used for squirting in this process are made of platinum.

An extension has been granted to the original inventors for their patent right in this country, so that although the patent dates from the year 1892 the rights are still in operation.

Cellulose silk process.—This product is manufactured under the Thiele patents at the English works at Great Yarmouth. Yarn is being produced down to 35 denier, or even lower. The process is a modified copper-ammonia one, but there are many secret details connected with the manufacture. In spite of the relative fineness of the yarn, the number of filaments is greatly in excess of other makes which are on the market at the present time.

Cellulose acetate process.—Several patents have been taken out in this direction, notably by the Bayer Company, the Badische Company, and the Donnersmarck Company. They chiefly deal with the control of the methods of preparing the cellulose acetate. The question of a suitable solvent seems to present a great difficulty, although it is stated that there are many derived acetates, and that some of these are soluble in alcohol, or pyridine; but chloroform seems to be the chief solvent. Quite recently (Eng. Pat. 6,654 of 1909), it has been claimed that formic acid is a satisfactory solvent. If this is so, a distinct advance has been made.

Gelatin and Casein Processes.—Very little has been done in this direction from the commercial point of view. In 1897 Millar patented a gelatin process (Eng. Pat. 2,713 of 1897); in 1907 Mugnier used vegetable albumins with the addition of borax, and Jannin patented the use of a solution of gelatin, glycerin, and formaldehyde in 1904 (Fr. Pat. 342,112). Casein was used by Chatelineau and Fleury, Timpe, and Todtenhaupt, but little has been heard of these processes.

Recovery of solvents.—The recovery of raw materials used as solvents is an important step in the nitrocellulose and copper-ammonia processes. It is, so far as I know, of small importance in the manufacture of viscose silk. In the first case, the recovery of alcohol, ether, or acetone from the air is important from the cost point of view, but is a difficult operation. Exactly what proportion is recovered has not been disclosed.

Quite recently the Tubize Company have patented the absorption of the alcohol and ether vapor in sulphuric acid of 62 deg. B. at 20 deg. C.

In the copper-ammonia process both the copper and the ammonia can be recovered by known means. When the precipitating liquid is of an acid nature, electrolytic methods are available for the removal of the copper, leaving the ammonia behind in solution. This solution may be used for manuring purposes, or the copper may be precipitated as sulphide.

Recently applications of the known reducing action of glucose have been brought forward in the case where the precipitating solution is of an alkaline or caustic nature. The addition of glucose to the precipitating bath throws the copper out of solution almost immediately, and the precipitating solution has a much longer life. This process works well in practice.

These few remarks will indicate some of the methods adopted in different cases, dealing with this important branch of the manufacture.

Machinery.—Each process has its special requirements as regards the machinery employed, and these have been naturally met in various ways. Companies working the same process in different countries differ materially in actual methods. In addition to this there is a mass of detail, which in many cases is not protected in any other way than that of secret working, and may be confined to the working of a single factory. Under these conditions I can only indicate one or two cases which may illustrate the methods adopted in the manufacture of these threads.

The first case is that of the original Chardonnet apparatus. This is of interest as showing in the original patent (Eng. Pat. 2,211 of 1886) the amount of detail already available in those early days. The thread passed from the jet, which had a bore of 1-20 to 1-5th mm., through a very short column of water and then on to the winder. When a thread broke, the broken end was seized by pincers and carried over guides to the reel to be wound. The pinions still ascending are cleaned by a rapidly revolving brush before they descend again to pick up any more broken ends. This movement is repeated several times a minute. Air heated to 85 deg. to 90 deg. F. is passed by supply and discharge conduits through the outer chamber. The vapors carried by the air might be "condensed and removed by cooling" and the air after warming returned to the apparatus. The so-called Topham turbine system of collecting and spinning the threads at the same time (Eng. Pat. 23,158 of 1900) is a good illustration of the methods adopted to overcome the difficulties in manufacture. The squirted thread passes over a roller and thence into a rapidly rotating box. The fibers or thread as they are fed in are twisted together and are caused by the centrifugal force to form a compact coil around the interior of the box and to be formed into hanks or skeins. If the boxes are deep a longitudinal reciprocating movement can be given to either the box or the funnel to make sure of the even coiling of the thread in the skein form. I have seen this apparatus at work on the Continent, and it certainly illustrates a very ingenious method of combining the skeining and twisting in one operation. It is, or has been largely used in the manufacture of artificial silk. It reduces the strain on the newly formed threads to a minimum.

A third example is that of one of the more recent patents dealing with modifications in the Thiele "two-solution" process of spinning. (Dreaper, Eng. Pat. 21,872 of 1908; see this J., 1909, 1246.) In this case arrangements are made so that the freshly squirted thread comes in contact with a precipitating solution which acts comparatively slowly, and then passes into a stronger one.

(To be concluded.)

* Paper read before the Society of Chemical Industry and published in the Journal.

The transmission lines of the Niagara, Lockport and Ontario Power Company running from Niagara Falls, Ontario, to Syracuse, New York, have recently been provided throughout their entire length with a device, designed by Mr. L. C. Nicholson of the Buffalo office of the company, which is said to afford excellent protection against lightning troubles. A metal ring is supported on the frame of the tower just below the lower petticoat of the insulator in such manner that the distance between the line wire and the ring is shorter than the distance between the line wire and the insulator pin. The ring, which surrounds the insulator, and is larger in diameter than the latter, is thoroughly earthed to the tower, so that the tendency would be for a lightning discharge to jump the shorter distance between the line and the ring in preference to the longer route to the pin and thence to the ground. Moreover, the arc, if there be any, in crossing the path thus formed, is kept away from the insulator, and trouble due to arcing is minimized. The device is claimed to have met all expectations.

WRIGHT VS. PAULHAN.

EXTRACTS FROM AFFIDAVITS AND JUDGE HAND'S DECISION IN THE CASE OF THE FARMAN AND BLÉRIOT AEROPLANES.

ON February 17th, Judge Hand, of the United States Circuit Court, Southern District of New York, rendered his decision in the case of Wright Brothers vs. Louis Paulhan, and granted a preliminary injunction pending adjudication of the Wright patent. M. Paulhan was required to put up a $25,000 bond for one month in case he wished to continue making exhibition flights in the United States. He ceased making such flights, returned to New York, and succeeded in getting the requirements changed to a weekly bond of $6,000. Meanwhile, on February 26th, Mr. Curtiss succeeded in obtaining a re-hearing in the Wright case against him, in which a preliminary injunction was issued also.

Below are given extracts from some of the affidavits put in by the defense (for which we are indebted to Aeronautics), and a part of Judge Hand's decision:

When Paulhan arrived in this country, papers in the suit were served upon him. The next day Judge Hazel's opinion in the Wright-Curtiss suit was rendered and that afternoon the Wright Company got an order to show cause why Paulhan should not be enjoined from flying. This motion was argued before Judge Hand on February 1st and 3rd.

At one point in the hearing on the latter day, Wilbur Wright himself took up the argument and was so convincing that the Court announced his intention to immediately grant the injunction, but a question to Wright by Israel Ludlow, who is associated with Clarence J. Shearn in Paulhan's defense, raised the point of reasonable doubt whether the Farman machine operates like the machine of the Wright patent.

HOW WRIGHT'S MACHINE TURNS CORNERS.

An affidavit of Lieut. F. E. Humphreys, of the Signal Corps, who has flown the government machine, was put in evidence by the Paulhan side. The Wright attorneys also put in a Humphreys affidavit which amplified the statements made in the previous one.

Lieut. Humphreys' operation of the Wright machine is described by him as follows, as regards turning:

"In turning in flight the outer wing is elevated by increasing the angle on the side opposite to that to which it is desired to go. Elevating the outer wing is essential to prevent the aeroplane from skidding. The rudder is turned in the direction to which it is desired to go, which is also the side having the smaller angle of incidence. Therefore, the wing with the greater angle rises and advances instead of being retarded as would be the case if the wings were warped without any movement of the tail. Technically considered, there are differences between the action of the vertical rudder of the Wright machine and the rudder of a ship, since in correcting lateral balance by warping the wings the rudder may be turned to one side to correct difference of the right and left wings, without causing the machine to turn. As the rudder is turned, the wings are warped. Then the angle of inclination on the outer side is brought back slightly past the normal so as not to continue the transverse inclination of the aeroplane further. Usually, during the turn the larger angle is on the same side as that to which the rudder is turned with reference to the longitudinal axis of the machine. To restore the course of a straight line, the rudder is brought back to a neutral state, and the aeroplane rights upon a horizontal balance. By the instruction of Mr. Wright, I aim to keep the machine skidding slightly outward when circling so that the rudder may be receiving a pressure on the side opposite the wing with the greater angle even when turned slightly toward that wing as compared with the longitudinal axis of the machine, since the axis of the machine and the relative wind do not quite coincide. This is the rule in all cases unless unforeseen and unusual occurrences interrupt the operation.

"The warping of the wings and the turning of the rear rudder is moved by one lever, which has two motions; that is, a forward and backward motion from front to rear to turn the rear vertical rudders from left to right; and the right and left transverse motion to increase the angle in each section, right and left respectively; that is to say, if one wants to make the larger angle on the right the lever is moved to the left. This enables the operator, in turning a corner, to increase the angle of inclination on the outer wing section and to elevate the same; or, should it become necessary, as sometimes happens to be the case from disturbing wind currents, the operator may increase the angle of inclination of one side, and at the same time swing the rear rudder in the direction of that side of the machine whose angle has been made smaller. All the way around the curve the position of the rear vertical rudder and the warped surfaces vary; one state of affairs requires a different position of the levers from another. In the last flight I made on the machine, which was in company with Lieutenant Lahm, the machine was flying close to the ground, when an attempt was made to raise the depressed wing by increasing the inclination of the low wing and decreasing the other, but owing to failure to promptly set over the tail toward the wing whose angle had been decreased, the depressed wing refused to rise and struck the ground, causing damage to the machine."

HOW PAULHAN TURNS.

In an affidavit submitted by Paulhan he says that though the "rear rudder is sometimes moved toward the angle of least incidence at the beginning of a sharp turn," it would be "suicidal to connect the rear rudder with the warping of the wing so that the rear vertical rudder would always be turned to the side having the least angle of incidence or to have the rear vertical rudders in any degree controlled by the movement of the aileron or the warping of a wing." He goes on to say:

"Under one condition an operator might increase the angle of incidence considerably on the right and move the rudder slightly to the left. Under different conditions he might increase the angle of incidence slightly on the right and move the rudder considerably toward the left. Under other conditions he might increase the angle of incidence on the right and move his vertical rudder to the right. For example, suppose the aeroplane to be proceeding northerly with the right wing tilted up at an angle of 10 degrees or more and to be suddenly struck with a gust or air current from the east. The operator would instantly bring his rudder toward the east or the angle of greatest incidence so as to swing or steer the aeroplane into the head of the wind. It is to steer with, maintain and control direction that the vertical gear rudders are used, and it is only occasionally as in turning a sharp corner that the rudders are used by turning them to one side or the other at the same time that the angle of incidence is changed and even in those cases, as above stated, whether the rudders are turned toward the angle of least incidence or the reverse depends upon the condition of the moment. The all essential requirement for safe and practical operation is that the operation of the vertical rudders shall be wholly independent of and distinct from any wing warping or increasing angles of incidence by means of ailerons or otherwise.

"In turning a corner in the Farman biplane machines . . . or in any aeroplane that I am familiar with, it is not at all essential to use the aileron so as to increase the angle of incidence on the outer edge in turning a corner. There are circumstances in making a turn in such machines and in a straightaway flight when the operator would use the aileron or warp the wings without turning the rudder at all and very often the rear vertical rudders are used without any interference with the ailerons or without any wing warping.

"If for some cause such aeroplanes move obliquely to their longitudinal axis, in other words, skid, the use of the rudder alone will correct the aeroplanes' equilibrium and bring them back to their normal line of advance. The operator can make a complete turn by the use of the rear vertical rudders alone and without using either ailerons or warping to correct horizontal equilibrium. The rear vertical rudders have a most powerful turning effect in all cases. In making a sharp turn the outer end of the aeroplane may be tilted up and a new plane of movement established which may be at an angle of ten or more degrees. The tendency of the rudder during such movement is to swing the tail to the outer side of the turning arc with great rapidity.

"Where one side of the Farman biplane . . . is depressed or tilted downward the side which is depressed tends to move more slowly and the aeroplane turns in the direction of the depressed side."

PAULHAN SAYS WRIGHT PATENT OF MACHINE IS IMPRACTICABLE.

Paulhan states in his affidavit that he is informed that the Wrights have abandoned the system described in their patent and now build aeroplanes with absolutely independent action and control of rudders and wings. He says:

"An aeroplane with flat sustaining surfaces is useless. To get lifting power the surfaces must be curved and the curve is a matter of careful study and adjustment and is not any mere curve that may be produced by a sagging or bellying canvas. As for having the rear vertical rudder controlled by and dependent upon the movement of the rope or lever that changes the angle of incidence, that is utterly impracticable and extremely dangerous. . . . Under such conditions a gust of wind or sudden air currents from the side of the aeroplane that was tilted up would inevitably upset the machine unless the rudder could be quickly and sharply turned toward the wind so as to steer the machine around into the face of the wind."

PRIOR ART.

That the Wrights admitted the prior art in the file wrapper of their application for patent is claimed in the quotation following:

"We are aware that prior to our invention flying machines have been constructed having superposed wings in combination with horizontal and vertical rudders."

After mentioning the airship patent of Lewis A. Boswell, September 24th, 1901, the Mattullath patent application filed January 8, 1900, and the D'Esterno, Mouillard, Le Bris and Ader machines discussed in Mr. Chanute's "Progress in Flying Machines," an affidavit is submitted signed by Dr. A. F. Zahm. In this he refers to his paper read before the Chicago Conference on Aerial Navigation in 1893, and printed in the proceedings in 1894 in which Dr. Zahm suggests moving the slats of the Phillips machine on either side thereof so as to present one side of the machine at a greater or less angle of incidence than the other to "arrest all pitching, rocking, and wheeling." He goes on to say:

"In general, at the close of the nineteenth century all essential principles and contrivances of pioneer flight were well worked out except one—a suitable motor. . . . A light automobile motor appeared in the later nineties and promptly thereafter followed the dynamic flyer. . . . The essential elements of aviation, barring the motor, had been clearly worked out. No further need to prove feasible the heavier-than-air machine, for that had been done repeatedly. Scientific design and patient trial, not physical research, was the chief demand. Further research would improve the aeroplane, but not bring it into practical operation. The aeroplane was sufficiently invented. It now wanted, not fastidious novelty, but concrete and skillful design, careful construction, exercise in the open field.

"The first person, to my knowledge, to apply for a patent involving the three-rudder system control was Hugo Mattullath, who in 1899 showed me, and many others, plans for an aeroplane having a vertical rudder in the rear and lateral steering planes on either side, fore and aft, of the main body, so disposed that he could cause the aeroplane to turn about either of the three rectangular axes at will."

There is very little that I should wish to add to Judge Hazel's opinion in the case of the Wright Company vs. Curtiss were it not for the ardor of the defendant's counsel and their insistence that a different showing has been made here. In view of the seriousness of the contest, I feel obliged to give my own reasons for this decision.

The defendant says that he does not infringe the patent because he does not use a device which automatically always presents to the wind that side of the rudder nearer the angle of lesser incidence; and that if the patent be construed as merely a combination of a vertical rudder with a device for creating a differential in the angle of incidence of the rear marginal edges of the plane, it is not a novel discovery, but was anticipated in the art. Therefore, the first consideration must be the proper construction of the contested claims of the patent in suit.

Claim seven is the main reliance of the complainants and that is as follows: "7. In a flying-machine, the combination, with an aeroplane, and means for simultaneously moving the lateral portions thereof into different angular relations to the normal plane of the body of the aeroplane and to each other, so as to present to the atmosphere different angles of incidence, of a vertical rudder, and means whereby said rudder is caused to present to the wind that side thereof nearest the side of the aeroplane having the smaller angle of incidence and offering the least resistance to the atmosphere, substantially as described." The specifications and diagrams upon which this claim was allowed after a pendency of three years in the Patent Office, showed the tiller-ropes of the vertical rudder attached to the rope which ran along the rear of the lower plane, in such wise that when the marginal parts of the two planes were warped as indicated, the rudder was turned toward the margin which had the lesser angle of incidence. Moreover, there was a constant proportion between the degree of deflection of the rudder and that of warping of the plane. The Blériot and Farman planes, which the defendant uses, do not

have the combination described and the complainants have in fact at times abandoned it. It is therefore a very sound contention that if the connection between the tiller-ropes and the warping device in a constant proportion, be an essential element in the combination patented, the planes which the defendant uses are in no sense infringements, and the case need not go into the question of the validity of the complainant's patent at all.

To an intelligent understanding of the invention and the question of how essential is the attachment of the tiller-ropes to the warping rope, the method of maintaining equilibrium under the patented combination must first be set forth. Assume an aeroplane with or without dihedral sustaining surfaces to be propelled through the air, having the combination specified, and also suppose the left wing has been accidentally depressed. That in itself will result, as all agree, in starting a revolution toward the left. This is the resultant of two motions, first, the forward motion of the plane, and second, the motion at right angles caused by the sliding of the machine laterally in its own plane and over the successive columns of air. The resultant is precisely analogous to any planetary motion. This resultant is accentuated by the movement of the center of pressure toward the depressed lateral margin, giving a greater leverage to the propeller nearer the elevated wing. Also, the vertical rudder becomes transverse in its reaction to the lateral motion of the aeroplane and consequently, the rudder is pushed up, and by its leverage further turns the direction of the plane to the left.

Thus the machine will begin to revolve to the left; moreover, this very motion will cause the right wing to be further elevated, because of the increased drift, or head resistance caused by its increased speed, and the decreased drift against the left wing, caused by its diminished speed. Thus in turn the initial depression creates a revolution, and that in turn an increased depression with its corresponding acceleration of revolution, so on co-operating till the machine will swoop downward to the left to its entire destruction.

The first part of the patented combination for correcting the depression of the left wing is to increase the angle of incidence upon the left side, so increasing that component of the drift which is opposite to the action of gravity. However, contrary to the assumptions of earlier speculators, this alone has a precisely contrary effect to what might be expected, because, although the lifting component of the drift is increased, the head-resistance is much increased and this decreases the velocity of the left wing in greater proportion than the increase in the angle of incidence tends to raise it. That revolution, already initiated by the very tilt itself, is therefore increased by the differential in the angle of incidence between the two margins. The right wing, which has thus an added velocity relatively to the left wing, will, in spite of its lesser angle of incidence, rise more rapidly than the left wing. Unless the revolution be corrected the increased angle of incidence will, therefore, remain ineffectual to restore the balance, but will rather further disturb it, and it is therefore necessary that the rudder is a part. The statute authorizes such an incounteracting the revolution. When this is done, the increased angle of incidence on the left wing becomes effectual and the left wing rises, so restoring the equilibrium of the plane. It is the combination of a differential in the angle of incidence with a rudder which operates against the side of lesser angles which produces this result.

Now to come back to the connection of the tiller-ropes to the warping mechanism. This is, of course, one "means whereby said rudder is caused to present to the wind that side thereof nearest the side of the aeroplane having the smaller angle of incidence, and offering the least resistance to the atmosphere." Literally considered, tiller-ropes under the independent control of the operator are equally such a means. But the invention is not of a machine; it is not an invention of this means of so turning the rudder, but it is an invention of a combination of which this action of the rudder is a part. The statute authorizes such an invention and if the combination be not a mere aggregation of old elements, as I shall try to show hereafter, then the precise means is of no consequence. In the patent in suit any skilled operator, who may serve *pro hoc vice* for a "skilled mechanic," finding the automatic connection unsatisfactory, would at once disconnect it and attach the tiller-rope to a lever or to a foot pedal which he could directly control.

As the Examiner said in his letter of July 14th, 1903, it is merely a matter of taste to attach the tiller-ropes to the warping rope. The machine would be changed, but the combination would remain, because there would remain the means of causing the rudder to operate on the side of lesser incidence. The defendant urges very vehemently that the means must be the means specified. All that the specifications need contain is so clear a description that any skilled mechanic may use the invention. Where the change is only an obvious modification of the means specified, and a modification which retains each element of the combination contributing the same effect as before, the claim is not too broad which includes the modification.

Of course, were the invention an advance over a prior art which had progressed already to the combination without any automatic movement of the rudder, then the claim must have been limited to the precise specifications. This would be because only when so limited would the patent be an invention at all and the construction would be necessary *utres valeat quam pereat*. The defendant insists that this is the case with the patent in suit, and I shall consider that contention later. Assuming for the present, however, that the patent need not be so limited to be saved, then it becomes a pioneer, and as such under the well-known rules is entitled to a broad construction.

Therefore, viewed first as a combination, not a machine, and second, as a pioneer patent which advances by more than just the degree of an automatic connection, I cannot agree that it was not a fair equivalent to operate the tiller-ropes independently by a mechanism under the direct control of the aviator. That connection was not essential to the three-rudder system of control.

The question next arises whether the fact, which I must assume from the affidavits, that the rudder under some circumstances of aviation is turned toward that side of the drift nearer the angle of greater incidence relieves the defendant of infringement.

In this connection, the distinction must be carefully observed between turning the rudder toward that side of the main longitudinal axis which is nearer the angle of the lesser incidence, and presenting to the wind that side of it which is nearer that angle; the latter being the essential specified. While the two might readily be identified, a moment's consideration discloses that they will be quite different whenever the plane is itself turning in either direction. For example, if the plane is turning to the left in a circle radius is r and has itself a total length longitudinal axis, s, then the rudder, if free, would stress at an angle to the inside of the axis whose tangent is $\frac{a}{r}$; that is $1 \frac{a}{r}$. That is to say, the sharper the turn, and the farther the rudder is set aft of the plane, the more will the rudder in turning stream inward, and apparently the operator will be putting the rudder toward the angle of greater incidence, although, in fact, the rudder may be sustaining a wind pressure from the opposite side.

However, I should have no right upon this motion to interpret the defendant's affidavit as limited to the phenomenon I have described, and I shall therefore assume that the rudder under his operation at times presents to the wind that side nearer the greater angle of incidence. To determine whether this indicates that the defendant does not infringe, we should look at the purposes of the invention. These are stated to be (page 1, lines 16-24):

"The objects of our invention are to provide means for maintaining or restoring the equilibrium or lateral balance of the apparatus, to provide means for guiding the machine both vertically and horizontally, and to provide a structure combining lightness, strength, convenience of construction and certain other advantages which will hereinafter appear." The question is not whether the defendant upon occasion may not find it proper or even essential to turn the rudder toward the greater incidence, but whether he uses the patent combination. For example, if the patent were for an automatic device, it would be no answer to say that the defendant used it only intermittently. This combination is in fact for a great part of the time used by the defendant "to maintain or restore equilibrium." If at times he avails himself of other methods, that is nothing to the purpose and I may disregard it.

As a method of restoring equilibrium the defendant has not shown that the rudder can be turned toward the greater angle. All he has shown is that in making a sharp turn he puts his rudder to the opposite side. During such a turn he may doubtless for a short time abandon his equilibrium, restoring it when his direction has been changed, at which time if it remains disturbed, he must restore it by the use of the patented combination, in short, his evidence going no further than to show that he may execute certain maneuvers during which he can safely for a short period abandon the maintenance of his equilibrium. That in no sense affects the question that to maintain or restore it he must eventually resort to the method specified.

Much discussion has arisen as to whether the defendant's rudder is not in fact a "steering device." I concede that it is when free from the warping device at once an essential part of the combination and also a "steering device" and that the complainants in some of their machines at times use it strictly as such. It is also true that when connected to the warping ropes it could not successfully be used as a "steering device," as Toulmin says in his letter of July 11th, 1904. The question is whether the combination specified is not actually used, even though the tiller-ropes are detached and the rudder thus made susceptible of being used to steer as well as to maintain the equilibrium of the plane. I think it is none the less the fact, when the ropes are detached, that the rudder when being used to maintain or restore the equilibrium must be used in the combination as specified and precisely as specified. It does not seem to me of consequence to say that by the detachment of the ropes, it may acquire an added function and that too of a kind not patentable.

As an illustration of my idea, suppose that there were two rudders, one with the tiller-ropes fixed to the warping mechanism and one free so as to be used simply for steering. In such a case no one could say that the addition of the second rudder would affect the combination. The infringement has simply dropped the automatically connected rudder and made the free one serve in both capacities. Is that not the adoption of an equivalent? The rudder yet remains a part of the combination with its means of being put over to the lesser angle. It has acquired by a simple suggestion to the mind of the operator, an added use and a more varied power of adaptation in the combination. I should feel most unwilling in a patent of this character to construe it so narrowly as to exclude the modification from the purview of the patent.

Nor yet is it of consequence that there may be other ways of maintaining equilibrium as by using the rudder alone. Anyone is of course free to use such other methods.

I think there is nothing in the further objection that the Farman machine has two ailerons or flaps instead of a general helicoidal warp through the whole plane. The use of such ailerons is an obvious equivalent, and the only possible question arises from the fact that the ailerons cannot be given any negative angle. However, the essential of the combination is a differential in the marginal angle and that is as well accomplished though the lesser angle can never be less than zero, as though it could.

Considering therefore that the complainants carefully avoid limiting themselves "to the particular description of rudder set forth," I think that the detachment of the ropes from the warping devices, leaves the patent substantially the same as specified.

Having now determined that the defendant infringes, it becomes necessary to determine whether the showing upon the prior art is such as to throw any reasonable doubt as to the validity of the patent. Moreover, in this consideration is involved the question of whether this is a pioneer patent, because if the three-rudder system of control be an invention of the complainants, then they are clearly entitled to have their patent take place as a pioneer.

Before, however, considering the prior art, it is necessary to determine first whether the patent is of a true combination or of an aggregation, and second whether it be merely a principle or abstract function, rather than a true invention.

As to the first, there can be no doubt whatever. The three elements are combined into an effect which is absolutely different from that which any one of them produces alone. The differential of angle instead of maintaining equilibrium would upset it. The rudder bearing upon one side only would not be sufficient. I am aware that the defendant contends that he can fly by steering alone, but I do not understand that he claims that in practice this can safely be continued permanently in machines of this type. In combination their result is not the aggregation of their separate results; it is the result of their mutual and antagonistic reactions. Even under the strict rule of Pickering vs. McCullough, 104 U. S., 318, here is fulfilled the requirements that the "old elements, all the constituents must so enter into it as that each qualifies every other; to draw an illustration from another branch of the law, they must be joint tenants of the domain of invention, seized each of every part, *per my et per tout*."

The question as to whether the combination is not merely of a function, is likewise plain. The combination is a definite adjustment of the material parts of a machine to secure a specified result. It is not the effort to patent a certain way of operating an aeroplane as the defendant insists, because the patent demands for its fulfillment certain physical parts in combination, able to work in the way prescribed.

The most plausible form of the defendant's argument is, I believe, as follows: "Given a plane with marginal flaps or a helicoidal warp and a movable rudder, the rest is a mere way of operating the machine. The marginal warp is not patentable, nor is the vertical rudder, hence the patent must be upon the operation of the two in conjunction, and that is not a proper subject of patent under the statute." The answer is that if the combination of elements were not new, the patent would be merely upon the method of operation, but the combination is, as I have said, quite new and the method of operating it need not be relied upon as the invention. No one before did in fact combine all these, and therefore no one gave to aviators the possibility of so operating.

(To be concluded.)

COUNTING PARTICLES SMALLER THAN ATOMS.

THE NUMBER OF PARTICLES PROJECTED BY CATHODES.

BY THE BERLIN CORRESPONDENT OF THE SCIENTIFIC AMERICAN.

If a glass plate, partially screened, be exposed for some seconds to a cathode discharge, the exposed portion does not at first sight seem to be distinct in any way from the protected portion. By merely blowing slightly on the glass a very marked distinction is produced by the condensation of vapor on the portion acted upon. The same phenomenon is produced by means of the vapors of alcohol, aniline, etc., but is particularly striking and easy to observe in the case of mercury vapors. (Figs. 1, 2, and 3.)

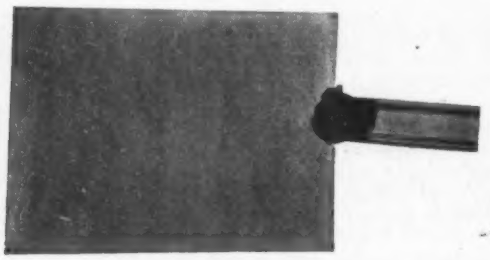


FIG. 1.—GLASS PLATE, THE CENTER OF WHICH HAS BEEN EXPOSED DURING FIVE SECONDS TO CATHODE BOMBARDMENT.

Dr. L. Houllevigue, professor at the University of Marseilles, has been led by observations of this kind to examine more closely the condensation of vapors on cold surfaces. To this end he used in the case of water vapor a flat box 7 millimeters in thickness, closed by two thin glass plates. Suitable ducts allowed either dried air or air saturated with water vapor (at a regulable temperature) to be introduced into this box. The precipitate of moisture in its interior could thus be made to increase or disappear at will, merely by actuating two India-rubber bulbs. Fig. 3 is a microscopic view of the phenomenon obtained with a ninety-five fold enlargement.

Even on an untreated glass plate the drops of moisture do not arrange themselves accidentally. If, after a first deposit, any trace of liquid be eliminated by a prolonged current of dry air, or by heating the plate to 150 deg. C., the drops on subsequent condensation will be precipitated exactly at the same points.

As the condensation of liquid augments, the drops increase in size, although no new drops are produced. On the other hand, when some of the drops grow sufficiently to come into contact with one another, they merge and the number of drops will thus decrease. Again, if the plate has been rubbed or wiped in a given direction, the drops will arrange themselves in the same direction. If a scratch be made on the glass with a diamond, the drops will arrange themselves along this scratch, and will crowd together in its vicinity. All these phenomena prove that under ordinary conditions the precipitate of moisture is connected with the presence of centers of condensation, dust particles, glass fragments, etc., on the plate. There is, however, no reason for supposing that these centers are electrified.

These experiments account for the phenomenon referred to in the beginning, viz., that a glass plate, exposed to the impact of cathode particles, is covered with a dense veil of moisture, under the action of vapors. In fact, each projected cathode particle becomes a center of condensation, and accordingly the core of a drop.

Another means of showing the existence of such centers consists in dipping into a chemical silvering bath a glass plate, part of which has been submitted to projection from a silver cathode. The metal deposit produced in the bath is much thicker in the region struck by the cathode projection.

The question arises: As the discharge, in addition to the cathode matter itself, projects a stream of corpuscles, it is not evident at first sight which of the two projections gives rise to condensation by the production of centers of attraction. The following experiment shows that only the first kind of particles possess this property.

Into a tube projects a brass anode. A silver disk cathode is placed at right angles to the axis tube. Opposite the cathode are placed two mica screens, pierced each by a central hole 4 millimeters in diameter. The one hole can be covered at will from outside by a mica disk, provided with an iron armature. Beyond the screen is placed, at 20 millimeters from the cathode, a crystal plate the diameter of which is 26 millimeters.

After producing in the tube a vacuum of 0.001 millimeter, the one hole is closed and the discharge of an induction coil is thrown into the tube (the closing current being eliminated by means of a spark micrometer). A horseshoe magnet is placed near the tube, so that the beam of cathode rays, limited by the other hole, is deflected sideways. The closed hole is then opened and the magnet is regulated so that the beam of cathode rays produces on the edge of the crystal disk a bluish fluorescence. After five minutes discharge, the operation is discontinued and the apparatus is dismounted. The silver projection from the cathode is then found to have formed a distinctly visible deposit on the wall of the tube. It has likewise produced a slight spot in the center of the crystal disk. Finally, at the point of the crystal which has been struck by the deflected cathode rays, a very slight spot, due, it seems, to the reduction of the crystals, is observed.

If the disk be exposed to mercury vapors, the central spot becomes distinctly visible. Microscopic study then shows the drops of mercury to be far more numerous on this spot, whereas nothing extraordinary

FIG. 12.—THE APPARATUS EMPLOYED.

is seen in the region *V*, struck by the cathode rays. This is borne out by the micro-photographs here shown. Figs. 4 and 5 correspond with the central portion of the spot, and show the number of drops per square millimeter to be 21,895 and 23,392 respectively; the third photograph (Fig. 6), taken in the central region, comprises 6,565 per square millimeter; whereas only 3,537 are obtained when photographing the region *V* struck by cathode rays (Fig. 7).

The latter rays are thus shown to play no part in the formation of liquid drops, and should be attributed to the projection of material particles. If each of these particles be considered as a center of attraction, giving rise to the production of a drop, a simple means is obtained for estimating the dimensions of those particles. By submitting two portions of the same glass plate to cathode bombardment, first, during a sufficiently brief interval of time t, for counting the number N of centers per square millimeter, and secondly, during a sufficiently long period T for obtaining a deposit of measurable thickness (e millimeters), the volume v of each projected particle is in fact readily found.

$$N v \frac{T}{t} = e.$$

The apparatus in which the cathode bombardment was produced consisted of a glass bulb, sealed to a metal plate, forming the anode, opposite to which a

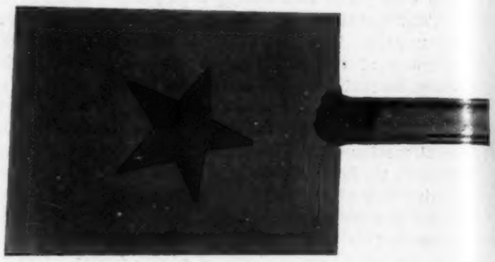


FIG. 2.—SAME PLATE AS IN FIG. 1 AFTER EXPOSURE TO MERCURY VAPOR.

plane circular cathode 60 millimeters in diameter of silvered brass was placed. The glass plate designed to receive the bombardment was placed on a metal plate, rigidly connected with the anode, carrying a steel point on which an aluminium disk with an aperture was placed. An iron plate glued to this disk allows it to be turned round from outside by means of a magnet so as to place the aperture in front of any given region of the glass plate. The anode is connected to the ground so that the glass is inclosed, apart from the portion left free, in a conductive case at the potential zero, which forms a perfect electrical screen.

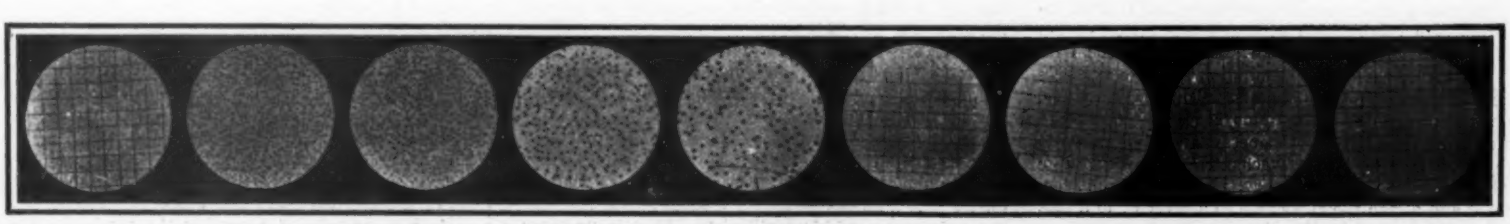
The plate is marked by a diamond scratch, dividing in into two sections. The plate is cleansed with acids and distilled water and glued to the anode.

After producing a vacuum of 0.001 millimeter, the induction coil is started with about five interruptions per second. The operation continues during ten to fifteen minutes, until a constant pressure of about 0.002 to 0.003 millimeter is obtained, which is a very important condition.

The five sections of the plate were exposed simply by placing the aperture in front of each of them. By causing mercury vapors to act on the exposed side of the plate, the different sections are revealed absolutely in the same manner as a photographic plate or a daguerreotype. On examining the plate under a microscope, the mercury drops are recognized distinctly, the regularity of their distribution being most striking. The number of drops per square millimeter increases regularly with the time of exposure. Fig. 8 shows a section of the plate submitted to no exposure, Fig. 9 a section submitted during six seconds to the impact of cathode projections, Fig. 10 the result of nine seconds action, and Fig. 11 the aspect of the plate after an action of fifteen seconds. The number of drops produced by the discharge is practically proportional to the time of exposure.

From the above it may be inferred that the number of centers of attraction produced during a given time on a given surface can be readily calculated by means of micro-photographic records. The same experiment allows the average mass of each projected particle to be calculated. The figure found by Houllevigue is 6×10^{-12}.

Supposing, according to Perrin's experiments, a hydrogen molecule to weigh 2.8×10^{-21} milligrammes, the mass of a silver molecule would be 3×10^{-19}. Cathode projections would thus be constituted by material elements twenty million times heavier than molecules.



FIGS. 3 TO 11.—MICROPHOTOGRAPHS SHOWING THAT DROPS OF MERCURY ARE NUMEROUS ON A SPOT BOMBARDED BY CATHODE RAYS.

ENORMOUS LOCOMOTIVES.

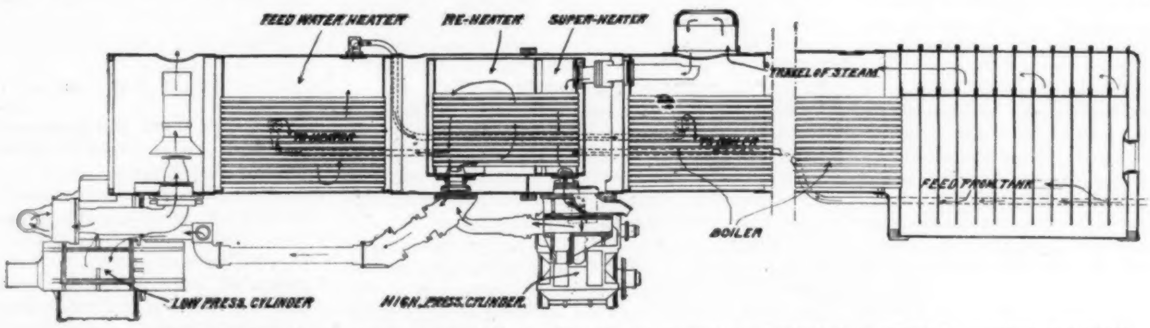
THE TWO NEW MALLET ENGINES IN ENGLISH EYES.

OTHER things being equal, the dimensions of a locomotive engine are fixed by the weight of the train it is intended to haul. But what fixes the weight of the train? This is one of those fluid problems to answer which is difficult, because there are so many unknown independent quantities. To say that it rests with the traffic manager is only to state that which is true in a very limited sense. There is, however, a prevalent belief, whether well founded or not is a point freely enough discussed, that it is cheaper to haul one train than to haul two each half the size of the one. So far as appears at first sight, the only saving effected is in the wages of drivers, firemen, and trainmen. There are, however, incidental expenses which may, perhaps, be regarded as being the same, no matter what the size of the train, and therefore dependent for the amount solely on the number of trains, and not on their individual dimensions. Under this head may be classed the wear and tear of points and signals, the cost of telegraphic apparatus and of its use, delays due to necessary shunting, and so on. Furthermore, it is possible that the tare or non-paying load is greater when two trains are used than with a single train. In the United States the principle of one train one engine has been pushed for some years. Locomotives have grown larger and larger, and at last two noteworthy locomotives have been built which are really for the time being at least more monstrous than anything ever heard of before.

The Atchison, Topeka & Santa Fé Railroad system is of enormous dimensions. It runs through the States of Missouri, Arkansas, Texas, Kansas, Colorado, New Mexico, and Oklahoma. It has nearly 5,000 miles open, and affiliated lines bring the entire system up to 8,300 miles, or nearly one-third of the length of the railways in Great Britain. In a word, it serves the principal southern States of North America. Mountainous country is traversed by the aid of inclines of great severity. To deal with these much power is required, and for several years the largest locomotives in the world have been found on this railway system. Large as they are, they are held to be too small, and a passenger and a freight engine have been ordered from the Baldwin Locomotive Works for use on the Atchison and Santa Fé section of the system. The first of these, No. 1301, weighs 305 American tons with the tender, or 272.3 English tons. Both engines are constructed on a modified Mallet system. The front end of the passenger engine is carried on a four-wheeled truck, and next we have four coupled wheels driven by the low-pressure cylinders, which are 38 inches by 28 inches. Next come the high-pressure cylinders, 24 inches by 28 inches, turning six coupled wheels. Last of all are two trailing carrying wheels under the firebox. There are sixteen wheels in all. The driving wheels are 6 feet 1 inch in diameter. The tender is carried on two six-wheeled bogies. The tank holds 12,000 gallons, or nearly 54 tons, of water, and 4,000 gallons of oil. The freight engine No. 1700 differs very little save in detail and certain dimensions from

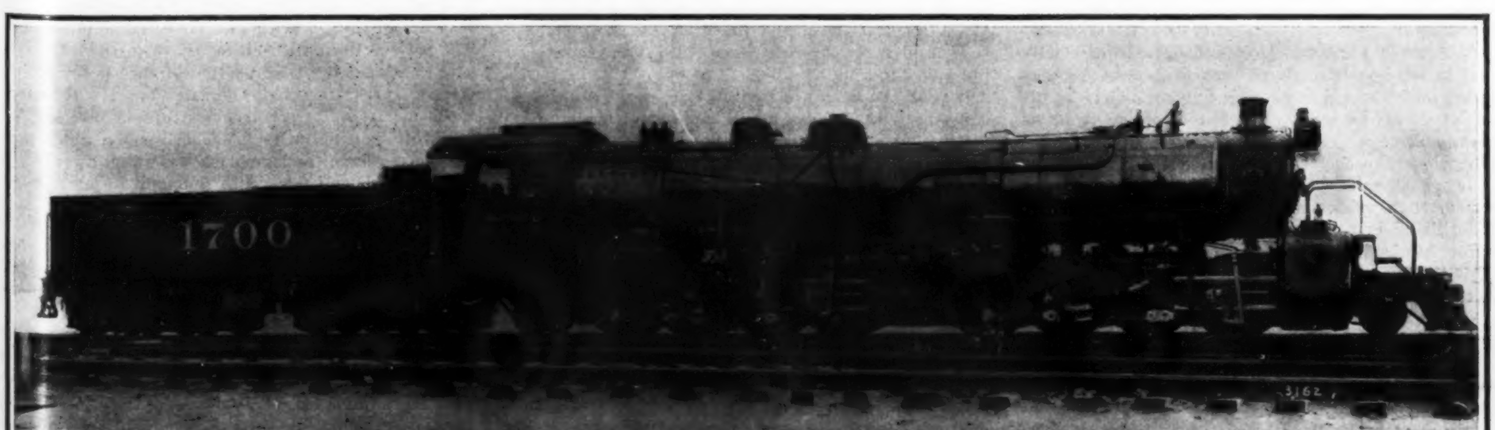
Weight of engine and tender, 305 tons. Tractive force, 26½ tons. Heating surface, 4,756 square feet. Steam pressure, 200 pounds. Superheating and reheating surface, 1,121 square feet. Cylinders: Two high-pressure, 24 inches by 28 inches; two low-pressure, 38 inches by 28 inches. Driving wheels, 73 inches.

THE MOST POWERFUL PASSENGER LOCOMOTIVE IN EXISTENCE.—A NEW TYPE.



. . . part of the heat in the furnace gases which is ordinarily lost through the smokestack, is utilized to superheat and reheat the steam on its way to the cylinders and to heat the feed water before it enters the boiler.

THE NEW BOILER WHICH SAVES 25 PER CENT OF FUEL.



Weight of engine and tender, 350 tons. Tractive force, 54 tons. Heating surface, 6,621 square feet. Steam pressure, 220 pounds. Superheating and reheating surface, 1,745 square feet. Cylinders: Two high-pressure, 26 inches by 34 inches; two low-pressure, 38 inches by 34 inches. This locomotive embodies superheating, reheating, feed-water heating, and compounding.

THE MOST POWERFUL FREIGHT LOCOMOTIVE. WEIGHT, 350 TONS

A NEW ERA OF THE AMERICAN LOCOMOTIVE.

the passenger engine. At the leading end is a two-wheeled truck. Then the low-pressure cylinders, 38 inches by 34 inches, turning eight, coupled, wheels. Next, the high-pressure cylinders, 26 inches by 34 inches; then another group of eight, coupled, wheels; and, lastly, a trailing pair under the firebox. The total weight of engine and tender is 350 American or 312.5 English tons. The tender is similar to that of the passenger engine. The freight engine alone weighs 207 English tons, leaving that of the tender 105 tons. Confining our attention to this gigantic machine, we have twenty wheels, each pair carrying an average of 20.7 tons. No doubt, the weight being unequally distributed, some of the axles represent over 25 tons. The working pressure is 220 pounds. The so-called boiler is a very peculiar structure. The barrel is 7 feet in diameter, and of enormous length. The tubes, over 20 feet long, open into what is termed a combustion chamber, in which is a superheater and re-heater combined, and thence to a feed-water heater, with 417 tubes, 2¼ inches diameter by 6 feet 8 inches long. The total heating surface is 6,621 square feet. The great merit claimed for this extraordinary machine is that it will do its work with 25 per cent less fuel than any previously constructed locomotives can do it with. Of course, we have only given the merest descriptive sketch of the engines.

We now come to certain considerations connected with their use. The tractive effort of the passenger engine is over 23 English tons, that of the goods engine is 48 English tons. Allowing 185 tons as the adhesion weight, by deducting 11 tons for each pair of carrying wheels, it appears that in order that this effort should be utilized, the coefficient of adhesion must be not far from 26 per cent, or nearly twice as much as it is safe to reckon on here. Assuming, however, that in some way this enormous effort can be exerted, we ask ourselves what must be the construction of the rolling stock which has to sustain it? Obviously, the stresses cannot be exerted through the wagon frames. A continuous drawbar must be employed, to which each wagon is secured either by springs or friction draft rigging. But what about the automatic coupler heads?—if, indeed, these are used. As to the loads to be hauled, only general statements are available. Let us take an incline of 1 in 40 and a gross load of 1,000 tons behind the tender. The resistance due to the incline would be 25 tons, and at 6.5 pounds per ton the rolling resistance would be about two tons more, or 27 tons in all, or nearly one-half that which the engine could overcome—on paper. This, then, seems, if we add 3 tons more for the resistance of the engine and tender, to be about the greatest load the engine can deal with; unless, indeed, as we have said, a coefficient of adhesion is available, unknown in this country.

We believe that we are correct when we say that these enormous machines would not have been built were it not for the belief that, as we have stated, they will do their work with an economy absolutely unprecedented. Engineers all over the world will look forward with interest to the publication of some definite information on this subject after sufficient time has passed to permit the collection of the necessary data. A similar method of construction may be carried out on a much smaller scale, should it be deemed justifiable. It should be kept in mind, however, that the first cost of these colossal locomotives must have been staggering. Being the first of their kind, with all patterns to make, we shall probably not be far off the truth if we say that between them they represent an outlay of $125,000 or $150,000, or, say, $250 per ton for the engine and tender complete in running order.

The interest on such a capital outlay is no trifle. Their depreciation and wear and tear and upkeep will probably represent a sum considerably in excess of that required for the same purposes by four engines, each of half the weight. In the absence of detailed information we can only express a qualified opinion. But it appears to us that the design is bad in that the cylinder power is much in excess of what the coefficient of adhesion will allow to be utilized. At this side of the Atlantic these engines only possess interest as mechanical curiosities, as no conditions exist under which it would be possible to use them. We have said nothing about the question of bridges and viaducts. We take it for granted that this is a matter which has been carefully thought out by the road engineers. We may add that even in this country mistakes have been made in this direction; but no doubt in the United States big risks are taken more freely than in this old kingdom.—Engineer.

THE SAD-IRON AS A DISINFECTOR.

HOW IT STERILIZES LINEN.

BY PROF. K. SVEHLA.

The prevention of contagious disease presents many and almost insuperable difficulties to the practising physician, especially in rural districts. One of the first conditions to be satisfied by a physician treating cases of contagious disease is that he shall not himself carry contagion to other patients. In my private practice I formerly endeavored to satisfy this condition by the following means:

After every visit to a patient suffering with a contagious disease I went home and changed my outer garments—and often my underclothes also. The suit of clothes was kept by itself in a special closet, and worn only in visiting the same patient, or another patient afflicted with the same disease. If I had several contagious diseases under treatment at the same time I always kept a special suit for each disease. When the treatment of the case was concluded, the suit was disinfected by formalin vapor in an air-tight cabinet. By this method I have hitherto avoided introducing any contagious disease into my own family or, I hope, that of any of my patients. It is, therefore, a good method of preventing contagion, but it is also a very troublesome method. A great deal of time is wasted in dressing and undressing, and in coming home after each visit to a patient. The method is also very expensive and, for all of these reasons, it would be almost impossible for a physician having a very extensive practice.

In order to simplify the process I began to wear, when visiting a case of contagious disease, a long linen gown which entirely covered my other garments. This gave security for the first visit but, before the second visit, it was necessary to sterilize the gown, in order to prevent the possibility of its contaminating my other garments. This danger is especially great in some exceedingly contagious diseases, such as scarlet fever, in which we are yet ignorant of the nature of the germ and the method by which it enters the organism.

In many households, sterilization by steam or by boiling was useless, as it could not be properly performed. I therefore sought a more convenient method of sterilizing, and ironing suggested itself as peculiarly appropriate.

For the purpose of testing the efficacy of ironing as a preventive of contagion, I made a series of experiments, in the hygienic laboratory of the Bohemian University of Prague.

In these experiments I employed four hollow sad-irons of different types. The first was heated by an inclosed mass of red-hot iron to 385 deg. F., the second by glowing charcoal to 428 deg. F., the third by a gas flame to 464 deg. F., and the fourth by an alcohol flame to 752 deg. F. while the flame was burning and 594 deg. F. after it had been extinguished. These temperatures were measured before the commencement of the experiments. Very hot irons cannot be used, on account of the danger of scorching the garments. The results obtained with the four irons were substantially the same.

I used a variety of fabrics, including linen, woolen, and cotton cloths of various textures, fine lawns, velvets, flannels, and materials used in making men's and women's garments. Every grade of thickness, from the lightest to the heaviest, was represented. These fabrics were artificially infected in various ways: by dipping them in contaminated water, by using them as dust cloths, by laying them in the beds of children suffering with various contagious diseases, by daubing them with pure cultures of the bacilli of typhoid, diphtheria, dysentery, etc., and by applying purulent matter to them. In every case a bacterial examination of the fabric was made before and after the experiment. In this way about 200 experiments were made, with the following results:

A single application of the hot iron sterilizes all fabrics superficially, and lawns, fine napkins, handkerchiefs, etc., throughout their thickness. The sterilizing effect diminishes as the thickness of the cloth increases. Heavier fabrics, such as the Russian linen of which my protective gowns were made, must be ironed at least twice on each side, in order to sterilize the interior. The hot iron may be passed ten times over one side without effecting complete sterilization. Very heavy woolen cloth cannot be thoroughly sterilized by ironing, by any method adapted for practical use.

In sterilizing materials as thick as Russian linen, which require the application of the iron to both sides, the ironing table must be ironed before the fabric is turned over, in order to prevent the table from conveying contagion from the unironed to the ironed face of the cloth. Fabrics which would be injured by direct application of the hot iron can be sterilized superficially by covering them with thin, moistened linen and ironing this until it is quite dry.

It might be imagined that superficial sterilization is of no practical value. For this reason I made an investigation of the depth to which such materials as velvet and flannel are infected by germs applied to the surface. I found that the infection remained purely superficial for a considerable period. Hence it may be assumed that, in most cases, the infection of garments by contact with the body of the patient or with sputum or pus is confined to the surface, and that all danger can be averted by ironing.

These experiments show that ironing is an easy and also an effective method of sterilization.

I now wear, in the presence of patients suffering with contagious diseases, long linen gowns, which are sterilized by ironing after each visit. The simple task of ironing can be required of every family and, in my experience, it has always been willingly and satisfactorily performed. It would be to the interest of every family to provide such a gown for the use of the family physician and to sterilize it by ironing after each visit. I believe that this innovation would be welcomed by all conscientious physicians, and would give the family an inexpensive safeguard against contagion.

The experiments also demonstrate the great hygienic value of ironing, in general. We come very frequently, and unwittingly, into contact with infectious matter, and our clothes are contaminated by coughing, sneezing, etc. We can protect ourselves and our families from disease by having our outer garments, as well as our underclothes, frequently and thoroughly ironed.—Translated from Umschau.

VELOCITY OF EXPLOSION OF GASEOUS MIXTURES.

Although the explosion of a mixture of gases is accomplished very rapidly, it does not take place simultaneously in every part of the mass, but is propagated from point to point with a definite velocity. The measurement of the velocity of the wave explosion has formed the subject of numerous researches. The German chemist Ernich has devised a lecture experiment by which the existence of a finite velocity of explosion can be shown, and the velocity measured. A steel tube about 30 feet long, with an internal diameter of 4 inches, is coiled up so as to occupy a minimum space, and its ends are brought within a few inches of each other and bent upward. Each end is provided with a cock, and to one of them is attached a device for igniting the contents of the tube by means of an electric spark. Above the ends of the tube a horizontal disk is mounted on a vertical axis, which can be caused to rotate at a known velocity—say 50 revolutions per second. The under side of the disk is covered with thick paper which is coated with lamp black. The apparatus is arranged so that the centers of the two ends of the tube are directly beneath the same radius of the disk. The tube is filled with an explosive mixture of gases, both cocks are opened, and the mixture is fired by an electric spark. A tongue of flame appears to dart at the same instant from each end of the tube, but the two white patches on the blackened paper, which mark the places where the lampblack was swept away by the outrush of burning gas, are found to lie on different radii of the disk. From the angle between these radii, the velocity of the disk and the length of the tube, the velocity with which the explosion travels along the tube in that particular explosive mixture can be computed with a fair degree of accuracy.—La Nature.

Although the use of telephones in mines is not of recent origin, states a contemporary, the advantages are, perhaps, hardly really appreciated until they have once been tried. Probably at no time in the history of mining has there been a greater demonstration of the great need of telephones in mines than at the Cherry coal mine disaster, in the United States. How many more lives could have been saved had the mine been fully equipped with telephones is entirely problematical, but it is certain that the number would have been greater had opportunity been afforded for communication between the rescuers and the entombed men.

Correspondence.

CARTAGENA—THE HEROIC CITY.

To the Editor of the SCIENTIFIC AMERICAN SUPPLEMENT:

I HAVE been reading with much interest the account of Cartagena which appears in the SCIENTIFIC AMERICAN SUPPLEMENT of January 22nd, 1910, the more so as I have been recently studying the various accounts of the British attack on that city in 1741. On page 57 you give a picture of Fort San Felipe, where, according to the title, Vernon's troops were defeated. I think this is a mistake. It was at Fort St. Lazar that they were repulsed. Fort San Felipe was on the island of Tierra Bomba, which was occupied at the commencement of the attack. The picture entitled "The Oldest Fortress in America" on page 57 looks to me as if it might be St. Lazar. It would be very interesting to me if you could find out that this is so.

Possibly the following few items about the American troops engaged may be of interest, and also the little photograph of an old plan of Cartagena which is in the British Museum.

When war was decided on with Spain, six regiments of marines were specially raised for Vernon's expedition, and prior to its departure were encamped in the Isle of Wight. This was late in 1739, and in the year following, according to Cannon's "Records of the Royal Marine Corps," "an additional regiment of four battalions was authorised to be raised in America, & the royal standard was erected at New York, as the signal post to which every volunteer marine was to repair. The field-officers & subalterns were appointed by the King, & the captains of companies were nominated by the American provinces. Colonel Spotswood of Virginia, was appointed colonel-commandant of the whole. It was supposed that, from climate, the natives of that continent were better calculated for the service to which they were destined, than Europeans. Their uniform was camlet* coats, brown linen waistcoats, & canvas trousers. This regiment, which was afterwards commanded by Colonel Gooch was considered as the Forty-Third regiment of infantry of the line."

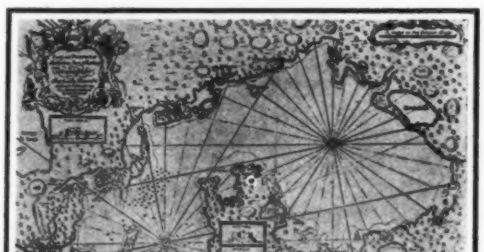
Elsewhere we read in a contemporary notice: "Three regiments of foot of a thousand men each, are raising with all speed in our American Colonies & will consist of natives or those inured to the climate. The Colonels, Lieutenant-Colonels, Majors & Subalterns are appointed by His Majesty & their general rendezvous is to be at New York, where the Royal Standard is set up. Their clothing is to be made here, which is camlet coats, brown linen waistcoats, with two pair of canvas trousers for each man."

"The American companies were chiefly raised by the interest & at the charge of their respective captains, of whom were members of the assemblies in the provinces where they resided: others lived upon their own plantations & had commands in the Militia. His Majesty was pleased to send to New York thirty young gentlemen, under the direction of Brigadier Blakeney, to serve in the corps as lieutenants: they had carried arms either in the old corps at home or in the Scotch Regiments in the Dutch Service, & were most of them cadets of good families in North Britain. The men were principally Irish Roman Catholics."

The regiments under Col. Spotswood joined Vernon's expedition at Jamaica on February 25th, 1741, and also a body of negroes who had been specially enlisted. Few American troops appear to have been landed until the outer harbor and the defenses at the entrance of the inner one had been captured, but two battalions of them with other troops were put on shore near St. Lazar on the 6th of April, three days prior to the unfortunate attack on that fortress. The troops detailed for the assault consisted of 500 men drawn from the grenadier companies of the various regiments under Col. Grant of the Fifth Marines, 1,000 marines under Col. Wynyard of the Fourth Marines—probably his own battalion—and a body of Americans who were sent into action without arms in order that they might carry scaling ladders, wool packs to fill the ditch, and hand grenades.† Brigadier Guise was in charge of the whole. As the attack had not been properly prepared for by bombardment and by a proper reconnaissance of the ground to be passed over by the attacking columns, some of which lost their way and came under a heavy flank fire from trenches of whose very existence they were ignorant, the assault was almost foredoomed to failure. The following is from an old report of the engagement: "The persevering & undaunted bravery of British Soldiers were never more displayed than on this day. They at last gained their points by intrepidly leaping into the entranchments & driving their enemies into the fort, which communicated by a drawbridge with the lines. Cooped up within a narrow spot, exposed to the grape shot & marksmen of the enemy above them, the situation of these brave remnants became desperate after the break of day. The unarmed Americans in the rear, many of whom were killed without possessing the power of resistance, dispersed, throwing away the charges committed to them." It should be added that there was a brilliant exception to this, for three of them in spite of the panic and confusion bravely pushed forward "on their own" with a ladder, by means of which a sergeant and a couple of men actually mounted to the summit of St. Lazar's ramparts, but only to be cut to pieces. The decimated stormers hung on in the bullet-swept space between the outworks and the fort itself till they received the order to retire, and were only extricated from a counter attack made by the garrison of Cartagena by the advance of the First Marines under the command of Col. Wolfe—the father of the hero of Quebec.

The Americans' loss must have been heavy, as only 323 re-embarked out of the two battalions that were landed, including 286 who were sick. The expedition afterward transferred its energies to Cuba, and we find Capt. William Hopkins of the American regiment issuing a proclamation for volunteers to enter, offering grants of land to all such families as were disposed to settle in that island. War and disease appear to have played havoc with these regiments, inured as they were supposed to be to the climate, for in a return of their numbers (exclusive of officers) issued October 5th, 1741, we find that the four battalions of Goodhe's Americans between them only mustered as fit for duty 210 sergeants, 197 corporals, 74 drummers, and 1,610 rank and file. It would appear very doubtful if more than a very small percentage of the men were Americans at all, for the "Irish Catholics," which it is said so many of them were, were not probably in very great abundance in the American provinces at that date.

Royal Marine Barracks, Chatham. COL. C. FIELD.



PLAN OF CARTAGENA.

* A species of rough material, a mixture of cotton and wool.

† As many of the American soldiers were suspected to be Irish Papists, it was thought admirable both by the General and Admiral to employ them principally aboard the fleet.—A Journal of the Expedition to Cartagena.

WATER-GLASS CEMENTS.

WITH all water-glass (soluble glass) cements the principal point to be attended to is to first heat the article to 212 deg. F. and then apply the water glass, previously liquefied by heating, with a warm brush to the surfaces which are to be cemented; the latter must be pressed closely together, or if necessary held in position by a cord tied round them. After drying for twelve to fourteen days at a moderate temperature the cement will be quite hard.

Cement for gas and water pipes is composed of water glass and amorphous silicic acid.

Cement for stone, glass, and porcelain.—According to Schwartze, a very hard-drying cement can be obtained from manganese, zinc white, and water-glass solution. Another good cement for these substances can be made by mixing powdered asbestos and soda-water-glass solution. The articles to be cemented should be moistened before applying the cement with liquid sodium silicate, and any cracks appearing during drying filled up with some of the cement slightly diluted. A good cement for glass and porcelain can also be prepared from 1 part of very finely ground and washed flourspar and ½ part of finely powdered glass with ½ part of soda-water-glass solution of 36 deg. Bé., these substances being mixed to a rather thickish mass. A thin layer of the mass must be rapidly applied to the surface to be united, and the parts pressed together. In a few days the cement will have become very hard.

For stone.—To cement stone, Lielegg uses a mixture of hydraulic lime and water glass.

Water-glass cement for iron and wooden water tanks, pails, etc.—These latter, if they are leaky, or if holes in the iron have been caused by rust, can be rapidly and easily again rendered water-tight by applying a cement prepared from water glass and amorphous silicic acid, coating the articles in question with a warm brush and drying the parts each time with the flame of a soldering lamp, which is done in a few seconds. The application should be repeated till the cement has formed a kind of crust over the spot where the crack or the hole was.

For steam pipes.—For cementing steam pipes instead of wrapping them in straw, which is apt to become rotten and to burn away, a very effective preparation is a mixture of chalk or lime dust and cow's hair or chopped straw with water glass.

For windows.—A mixture of chalk and water glass specially prepared for this purpose is the most reliable kind of putty for windows and preferable to the putty made of chalk and linseed oil hitherto used. Apart from the low cost, this water-glass putty has the advantage of drying in a few hours without any odor. Care should be taken, however, not to prepare more of the material than is required for the articles in question, otherwise it will become hard.

NEW INVESTIGATION OF THE RAINBOW.

ONE would think that every child would know how the rainbow looks, but that is by no means an incontrovertible fact. The question is solved so slightly by any answer that suits all cases that savants never tire of making it the object of further investigation. They determine with the latest results of their research that the appearance of the rainbow is endlessly various, but do not succeed in reproducing the colors of the rainbow with evident precision. They hope to attain better results with the new photographic systems.

Since Newton (1704) it has been assumed that the rainbow in the falling drops of rain is produced in the same way as a bright strain of light when sunshine falls on a prism. The fact is, however, that sunlight can never properly be called white, since the sun shines in a yellow mixed color which together with the blue light of the sky gives the white light of day. The mixed color produced from the light of the rainbow does not always bear some resemblance to white, for the reason that as the sun sinks at the horizon it steadily loses a greater measure of its violet, blue, and green rays; and in fact the appearance of the rainbow changes within far boundaries. When the sun rises and sets rainbows contain often only red rays, and rainbows colored with red and orange are no infrequent phenomena, indeed. With great frequency secondary bows are formed about the red rainbow which are far from being in consonance with the sequence of color. When diffraction occurs light of a given color gives a sequence of concentric bright bows which are separated by bright streaks. The outermost bow is much the brightest, and the successive inner or secondary bows rapidly show a diminished light. The distance of the bright rays from one another is by so much greater the smaller the drops are by which the light is diffracted. At the same time the bows are broadened, their light is weakened and their diameter decreased. The outermost and brightest bow of every color disposes itself, in accordance with the order of the spectrum, with the corresponding bow of other colors, but covers them in a measure, and by so much more the smaller the drops are, so that mixed colors appear. This occurs oftener among the secondary bows which have almost a pure, full color. If the rain drops have a radius of 0.1 millimeter, the first secondary sequence of color mingles with the inner violet parts of the chief bow and destroys the order of color as well as the distinctness of the secondary bows. Drops of medium size with a radius below 0.1 millimeter give a rather regular sequence of color as chief bow, in which blue is often not recognizable, then a dark interval, and thereafter secondary bows mostly in the mixed colors purple and whitish green. If the drops are very small, say with a radius below 0.03 millimeter, all the colors mingle and the rainbow appears white; only its outer border remains tinged with uncertain brown to yellow while its inner border has a violet cast. The bow is then broad and weak, and the likewise white secondary bows seldom are seen. Consequently it follows that the appearance of the rainbow is infinitely various in accordance with the ever different size of the drops of rain.

Most of the penny-in-the-slot machines are worked by "victimized power"; that is, power derived from the patron or "victim." The patron furnishes the muscle that works the mechanism as well as the cent that releases the operating machinery. Strange to say, the element of action on the part of the user is an important factor in the success of some of these machines. It appears that many of the users of these machines enjoy pulling a lever; they somehow feel that they are getting more for their money than if they simply pressed an electric button. A certain fortune-telling machine was first put out with an electric motor driving the machinery. The patron only had to deposit his coin and place his hand on a slab in which there are many projecting studs to have his fortune told. The machine did not prove to be a "money puller." The motor was discarded and a long lever was substituted, which must be pulled each time a fortune is told. This machine yields a handsome income. The experience adds to our knowledge of the curious phases of human psychology.—Machinery.

HALLEY'S COMET AS SEEN FROM THE EARTH.

THE POSITION OF THE COMET.

BY P. H. COWELL.

THE following table gives ecliptic co-ordinates of Halley's comet to two decimal places at intervals of four days through an arc extending from one end to the other of the *latus rectum* of its orbit. The zero of time is very nearly the moment of perihelion passage, and the figures apply approximately to any return.

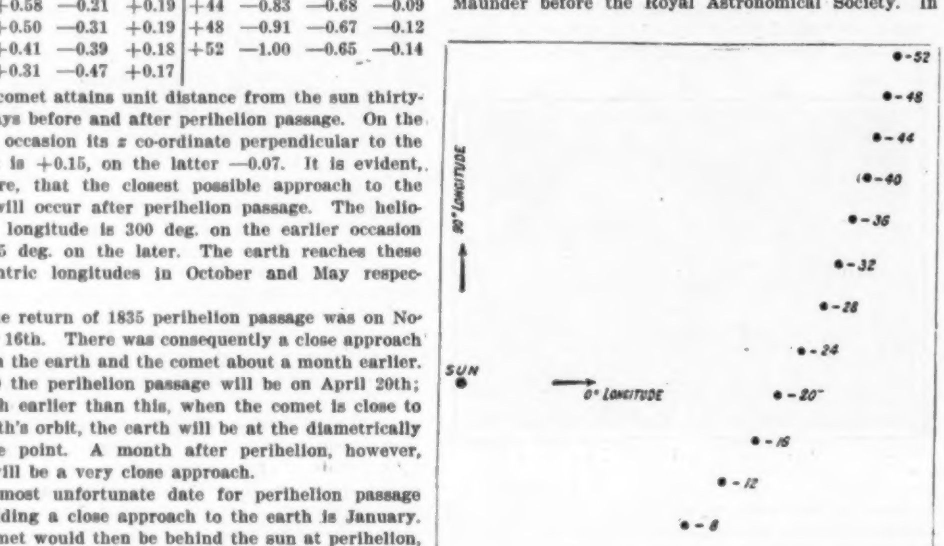
Day	x	y	z	Day	x	y	z
—52	+0.95	+0.73	+0.12	+ 4	+0.21	—0.54	+0.15
—48	+0.93	+0.64	+0.13	+ 8	+0.10	—0.59	+0.13
—44	+0.91	+0.55	+0.14	+12	—0.02	—0.64	+0.11
—40	+0.89	+0.46	+0.15	+16	—0.13	—0.67	+0.09
—36	+0.86	+0.37	+0.16	+20	—0.24	—0.69	+0.06
—32	+0.83	+0.27	+0.17	+24	—0.34	—0.70	+0.04
—28	+0.80	+0.18	+0.18	+28	—0.45	—0.71	+0.01
—24	+0.75	+0.08	+0.18	+32	—0.55	—0.71	—0.02
—20	+0.70	—0.02	+0.19	+36	—0.64	—0.70	—0.04
—16	+0.65	—0.12	+0.19	+40	—0.74	—0.70	—0.07
—12	+0.58	—0.21	+0.19	+44	—0.83	—0.68	—0.09
— 8	+0.50	—0.31	+0.19	+48	—0.91	—0.67	—0.12
— 4	+0.41	—0.39	+0.18	+52	—1.00	—0.65	—0.14
0	+0.31	—0.47	+0.17				

The comet attains unit distance from the sun thirty-nine days before and after perihelion passage. On the former occasion its z co-ordinate perpendicular to the ecliptic is +0.15, on the latter —0.07. It is evident, therefore, that the closest possible approach to the earth will occur after perihelion passage. The heliocentric longitude is 300 deg. on the earlier occasion and 225 deg. on the later. The earth reaches these heliocentric longitudes in October and May respectively.

At the return of 1835 perihelion passage was on November 16th. There was consequently a close approach between the earth and the comet about a month earlier. In 1910 the perihelion passage will be on April 20th; a month earlier than this, when the comet is close to the earth's orbit, the earth will be at the diametrically opposite point. A month after perihelion, however, there will be a very close approach.

The most unfortunate date for perihelion passage for yielding a close approach to the earth is January. The comet would then be behind the sun at perihelion, and more than an astronimcal unit away when crossing the earth's orbit.

On the present return the approach after perihelion



The diagram gives the position of the earth for six days in May; also the position of the comet on twenty-seven dates measured from perihelion passage in days. The line of sight is drawn for May 18th, twenty-eight days after perihelion, when the comet transits across the sun.

will be unusually close. The following table gives the ecliptic co-ordinates of the earth for the annexed dates:

Date.	x	y
1910, May 10th...........	—0.65	—0.76
1910, May 14th...........	—0.60	—0.80
1910, May 18th...........	—0.55	—0.84
1910, May 22nd...........	—0.49	—0.87
1910, May 26th...........	—0.43	—.090
1910, May 30th...........	—0.37	—0.93

When, therefore, the comet crosses the plane of the ecliptic twenty-eight days after perihelion passage (May 18th) it will be almost exactly between the earth and the sun, and the earth will probably be in the tail of the comet.

The closest approach at this return takes place a day or two later.

The closest approach possible would correspond to a perihelion passage about a week and a half earlier in the year than the present one.

It appears, therefore, that the date of perihelion passage at this return is most fortunately timed, and a fine display may be expected.

The comet's history has been traced back to 240 B. C., and it has very seldom returned to perihelion unrecorded; so seldom, in fact, as to suggest that in the exceptional cases the records have perished rather than that the comet in any circumstances can pass by unseen.

A tail twenty or thirty degrees in length is expected on the present occasion. It will be best seen at the end of May, and in England it will, unfortunately, be lower in the sky than in more southern latitudes. There will, however, be no difficulty whatever in seeing it in England, unless there is a prolonged spell of bad weather.

The approach to the earth is so close that an American astronomer has conceived the idea of weighing the comet by the deviation it produces in the orbit of the earth. We can hardly believe that the effect produced will be one-thousandth part of the least measurable quantity, but the speculation is most interesting in view of the fact that there are unexplained phenomena in planetary movements.

* Nature.

RECENT STUDIES OF MARS.

PERHAPS the most distinguished of European Martian observers is E. M. Antoniadi, whose name figured prominently in the recent discussion provoked by E. W. Maunder before the Royal Astronomical Society. In the Comptes Rendus, Antoniadi presents some observations of Mars made at Meudon. The planet was studied with the 0.83-meter telescope, with magnifications 320, 470, and 800 diameters, on thirteen nights from September 20th to November 9th. Among the most remarkable changes noticed since the opposition of 1907 were the return of Syrtis Major to its form of 1864 and 1877, the reappearance of Lac Mœris, and the formation of a multiple "island" in the eastern region of the Mare Cimmerium. At the same time the planet was partly veiled by mists or light cirrus, which lightened the indigo-gray tone of the "seas" and gave a yellowish hue to the "continents." The tint of these mists or cirrus varied, according to their intensity, from golden-yellow to dull white. Their effect sometimes in almost obliterating spots which had appeared black a few days earlier was very remarkable. Some fifty "canals" were observed. Adopting Schiaparelli's definition of a canal, as a grayish band or line in the regions called "continental," assuming all kinds of forms and longer than the more or less elliptic spots called "lakes," Antoniadi proceeds to divide the canals into eight classes. The list does not include the fugitive straight lines often also called canals, which are visible only a fraction of a second, and may be illusory. The observations do not confirm the existence of a geometric network of straight lines crossing in all directions.

Photography of the planet Mars was recently discussed before the French Academy of Sciences by A. de la Baume Pluvinel and F. Baldet. The photographs exhibited were obtained at the observing station on the Pic du Midi with a new double instrument comprising a reflector and a refractor (diameters 0.50 and 0.25 meters, focus 6 meters) on a very steady mounting. The focal image of Mars being only 0.8 millimeters diameter, an enlarging lens was employed, giving disks of 3 to 5 millimeters diameter. During September and October 80 plates with 1,350 images were obtained. At first Lumière violet and Guilleminot lactate of silver plates were used, but soon abandoned, as they rendered the ruddy surface of the planet uniformly gray without any detail. They show the polar caps distinctly, however. The southern cap is much reduced, but very brilliant; at the north pole, on the contrary, a region of the cap is seen which is rather far from the pole and deficient in brightness. The eastern border of the planet is brighter than the rest of the disk. After the first trials a yellow screen was placed behind the magnifying lens, and plates sensitive only to the rays passing through it were employed. Under these conditions the Martian snows act feebly on the plate; the south cap is still fairly intense; but the north cap, and also the white border on the eastern limb are invisible. On the other hand, the contrasts between the Martian lands and seas are very apparent and a considerable amount of detail is shown. The canals of the first order are visible on the photographs, such as Indus, Ganges, Axarxes, etc. No trace appears of the geometric network of fine lines seen by certain observers, the existence of which is disputed.

HAS THE CLIMATE OF NORTH AFRICA CHANGED SINCE THE ROMAN PERIOD?

THE fact that most forcibly impresses the traveler in Tunis, Algeria and Morocco, is the great development which Roman civilization attained in those countries. This fact is particularly striking in Tunis, and in the province of Constantine, in Algeria, which are filled with ruins of large Roman cities, extensive aqueducts and other works. The contrast with the poverty of the present inhabitants and the sterility of the ill-cultivated soil makes these evidences of former prosperity still more impressive. It has often been suggested that a change has taken place in the climate of North Africa, and that this alteration in the aspect of the country has been produced by a decrease in the rainfall and an elevation of the mean temperature of the region.

Gen. de Tiamothe came to the conclusion, according to Cosmos, that the climate of Algeria was marked by violent contrast, even before the appearance of man on the globe, during the geological periods of the pleistocene and the upper pliocene, and that the distribution of rainfall through the year was as unequal then as it is now.

A very long interval of time, however, extends between the pleistocene and the present epoch, and it would be interesting to know the character of the climate of North Africa during the flourishing Roman period. Dr. Leiter has endeavored to obtain this knowledge from historical data.

From such information in regard to the climate of the Mediterranean region as is given by Greek and Roman writers, it is impossible to draw any evidence that a change has occurred in the mean annual temperature, the fluctuations of temperature, the pressure of the atmosphere, the directions of the prevailing winds, or the amount of rainfall. The persistence of the cultivation of certain crops proves that the climate has not changed. The crops are evidently smaller than they were in Roman times, but this is due to the negligence of the present inhabitants.

The felling of forests could not have greatly altered the climate of so extensive a region. From the data which we possess in regard to the extent of the ancient forests it appears that the removal of lofty forests has been confined to the present territory of Algeria. Nor do the changes which have taken place in the fauna of the country indicate that the climate has become hotter or dryer. The disappearance of large mammals and the subsequent introduction of the camel have resulted from man's invasion of the region.

It should be observed, however, that Dr. Leiter's conclusion that the climate of North Africa has not changed since the Roman period, is at variance with those derived by Gantier from his travels in the Sahara and by Freydenberg from his study of the country about Lake Tchad. There can be no doubt that the climate of each of these regions has gradually become dryer.

HOW THE FLORA BUST WAS STUDIED.

THE COLORS OF THE BUST IN THE BERLIN MUSEUM

BY PROF. E. RAEHLMANN.

The question of the genuineness of the bust of Flora in the Kaiser Friedrich Museum in Berlin is intimately connected with the question of its antiquity. To me was assigned the task of ascertaining whether any reliable indication of the age of the bust could be obtained from the character of the pigments with which its surface is colored. The investigation was carried on with the aid of the microscope.

Small fragments which have become detached from the surfaces of paintings can be satisfactorily examined with a power of 200 diameters, if the illumination is good, especially when fresh surfaces of fracture are exhibited. On these surfaces it is possible to distinguish the successive coats of paint, which differ in color, density, and refrangibility.

It is often possible to distinguish, in the same way, the ingredients of a single coat of paint. This determination is particularly easy when the pigment is not dissolved, but consists of fine solid particles suspended in a transparent medium. Then it is possible to determine both the character of the medium and that of the pigment. The latter is identified by the size of the grains, their degree of transparency, and their form. Sometimes it is possible to determine the period in which the picture was made pretty accurately from the character of the medium and the optical and chemical behavior of the pigment. I feel confident that, with a further development of the method of investigation which I have employed, I shall ultimately be able to prove or disprove the authenticity of a picture by comparing the work with other paintings of the same epoch attributed to the same artist.

In the old days, every art period had its characteristic technique, which depended upon the education of the artist in studios, schools, and guilds. The Italian Dutch, and German painters of the Renaissance epoch employed mediums which they prepared with their own hands in accordance with traditional secret formulas. A close comparative study of old paintings indicates, however, that the artists of all these countries knew and used the same, or nearly the same, medium.

To this medium these old pictures owe their permanence and the eternal beauty of their colors. With the disappearance of the art schools and guilds, the formula of this medium was lost. Various substitutes were employed successively until the substances now in use were introduced.

When the surface of fracture of a freshly broken fragment of a painting of the Renaissance period is examined under the microscope with a power of about 50, we see a medium of peculiar, transparent, homogeneous structure, which presents an appearance entirely different from that of a fragment of a painting made within the last one hundred years. The character of this medium can be made out more clearly when it is colorless, that is to say, when it contains only insoluble solid particles of pigment and is not stained by dissolved pigments. In old pictures this condition is found especially in the blues (ultramarine and cobalt) and some of the reds (vermillion, red lead, madder, etc.) whereas the green and yellow pigments which were then employed were for the most part dissolved in the medium, which they consequently stained throughout. Hence, the old painting medium can best be studied in the blue and red parts of the picture. According to my experience, the best objects for study are blue articles of clothing in old paintings and carved wood figures of the thirteenth, fourteenth, and fifteenth centuries. In all of these cases the medium is of a brilliant, pale gray color, transparent in thin layers, and the pigment grains are seen completely imbedded in it. With a magnifying power of 200 or more, this medium is seen to be finely granulated. When a fragment of the surface of a painting of the fifteenth or sixteenth century is compared with one of the eighteenth or nineteenth century, the contrast is very striking. The modern fragment is far less transparent than the old one and is often quite opaque, almost entirely concealing the pigment grains, except those which are actually on the surface.

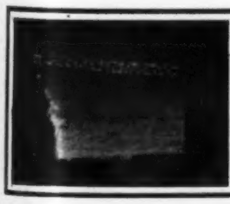


Fig. 1. Light Blue Pigment Layer Seen from the Back, and Bent to Show Cross Section.

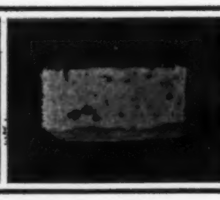


Fig. 2. Surface of Bust, Showing Large Red particles Imbedded in the Gypsum.

In accordance with this change in the medium, the layers of paint have gradually become thinner and thinner in modern times. In glazes this modern medium can be employed only in extremely thin layers, while old paintings show glazes which are quite thick, but which are yet sufficiently transparent to allow the underlying color to show through them.

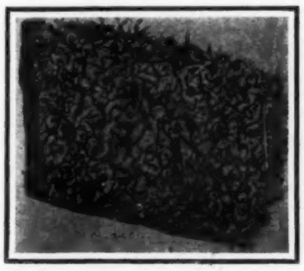


Fig. 3. Thin Section of the Hair, Seen by Transmitted Light and Greatly Magnified, Showing Brown Lichens and Scattered Red Grains of Madder.

In the determination of the technical construction of old paintings, it is necessary to consider, in addition to the layers of paint, the so-called ground, or first coat, which serves to attach the pigment layers to the canvas, wood, or plastered wall upon which the painting is executed. In old paintings this ground layer is very thick, and it is composed of various substances, the knowledge of which was handed down by tradition in the various art schools. This pasty foundation is very similar to the outside layer of antique wall paintings, from which it was very probably derived. The character of the ground layer differed somewhat according to the character of the material, wood, canvas, plaster, etc., upon which the painting was made. In the bust of Flora, the ground was determined by the coating of wax below it.

I received a number of small fragments of the bust of Flora, which has given rise to so much discussion. My task was to examine these tiny fragments, from the points of view explained above, and to compare them with the large number of fragments of art works of the Renaissance period which were in my possession. The fragments of the bust of Flora were especially suitable for this investigation because the painting is made on pure white wax. The first idea which suggested itself was that wax had also been used as the painting medium or vehicle, but it was found impossible to melt the pigment layer on a microscope over a glass flame. This experiment proved that the pigments were mixed with a special medium, although they were applied to a surface of wax. In the subsequent investigation,* this peculiarity was found very convenient, because it made it easy to detach the colors from the wax by immersing the fragments in ether. After several changes of the ether, the pigment layer separated completely from the wax, so that its under surface could be examined.

When the colors of the garment are thus examined from the back of the pigment layer, they reveal a grayish white substratum of considerable thickness, composed of two distinct layers. The under layer in contact with the wax is thin and contains fine fibers. Microchemical examination revealed the existence of a large proportion of albumen in this substratum (Fig. 1). Above this albumen ground is the pigment layer of the blue garment. In other fragments of darker blue color, a brownish red layer intervenes between the gray ground and the blue pigment layer. In other words, different grounds were used for light blue and dark blue.

The blue pigment itself is used in the form of coarse grains of various forms, some rounded, others angular. These grains are imbedded in a peculiar, transparent medium, which shows fine granulation when magnified 200 times. Hence this medium is very similar, both in appearance and optical behavior, to the old transparent mediums which I have already described. The distribution of the pigment in angular grains, furthermore, is identical with that of the grains of blue pigment in paintings of the Renaissance period. The fragments of the bust of Flora exhibited a particularly close resemblance to fragments of a painting by Marco Basaiti, a contemporary of Leonardo di Vinci, and also to the blue parts of wood figures of the preceding period. The method of applying the pigment—blue on reddish brown, for example—is quite similar to the methods of the Renaissance painters.

In order to study the brown color of the hair of the bust, I separated portions of the pigment layer from the wax and examined them with the microscope. Even with a low power, I discovered that the pigment was of vegetable origin and derived from a lichen (Rocella). The coarsely ground lichen served as the brown pigment, and the reddish tint of the hair was given by the addition of a transparent red color, probably madder, in grains and large flakes. Thin parts of the pigment layer of the hair, mounted in Canada balsam, exhibited the structure of the lichen very distinctly (Figs. 3 and 4). These lichens, which grow on the shores of Corsica and the Canary Islands, were much used in painting in the middle ages, but were later employed almost exclusively, under the names of archil and cud bear, in wool dying. Madder in very large particles was extensively employed in old paintings, but it is now ground very fine or dissolved in the medium. The lichen pigment found in the hair proved to be identical with the brown pigment mentioned above, which underlies the dark blue of the garment.

Small fragments of the bare surface of the bust proved upon microchemical investigation to be composed chiefly of gypsum. In places the brilliant white surface is diversified by coarse grains and lumps of a red pigment, probably madder, half concealed by a thin layer of gypsum (Fig. 2). Particles taken from all parts of the surface of the bust show traces of forcible removal of the color by scraping, washing, etc., as well as unmistakable indications of attempts at restoration. In some places the wax is quite bare.

The details of this painting show careful elaboration, especially in the blue garment, and a technique which is characteristic of Italian painting in distemper of the fifteenth and sixteenth centuries and German and Dutch works of the same period. Some of the materials employed, especially the medium, have not to my knowledge been used in painting within the past 150 years.—Translated for the Scientific American Supplement from Umschau.

Interesting tests of the porhydrometer have been recently made in Liverpool. It is an instrument for indicating the weight of cargo loaded into or unloaded from a ship. It consists merely of a float or "aerometer," connected to a steelyard weighing machine and immersed in a chamber filled with water, the chamber being in communication with the outside of the ship. The ship rises or falls as the cargo is removed or placed on board, and the level of the water in the chamber falls or rises correspondingly, so that the float is immersed to a greater or less depth, with a consequent alteration in its apparent weight. This alteration in apparent weight is read on the weighing machine, and from it the weight of the cargo is determined. In order that the reading of the apparatus may not be affected by the trim of the ship, it is necessary that it be fixed directly over the keel and in the plane of rotation of the hull, which generally will correspond approximately with the center of the ship. Also, it is necessary that the over-all length of the float and its chamber be greater than the total vertical movement of the ship between the "light" and the fully loaded conditions. The appliance is of a very simple nature, and there should be no difficulty in fitting it to existing ships. The inventor claims that it has been officially approved by the Italian government customs.

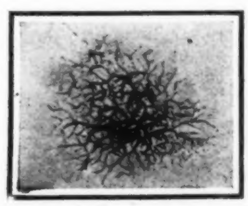


Fig. 4. The Lichen Pigment Washed to Remove the Ry Grains, etc.

THE NEW CANALS OF MARS.*

RECENT DISCOVERIES AT FLAGSTAFF.

BY PERCIVAL LOWELL.

The word "new" when applied to a celestial phenomenon may be used in either of two senses. It may mean new to earthly observation, i. e., one which has never been seen by human beings before, or, secondly, new in itself, that is, one which has had no previous existence. New canals on Mars in the first sense, though always interesting, and at times highly important, are no novelty at this observatory, inasmuch as some four hundred have been discovered here in the last fifteen years. When Schiaparelli left his great work, he had mapped about 120 canals; with those detected here since, the number has now risen to between five and six hundred. Each of the four hundred thus added to the list, however rich an acquisition at the time it first came to be noticed, was not necessarily otherwise remarkable.

To observe, however, a canal new in the second meaning of the word, one, that is, that had never existed anteriorly, and to prove the fact, is an astronomical detection of far-reaching significance for the bearing it has upon the whole Martian question.

On September 30th, 1909, when the region of the Syrtis Major came round again into view, after its periodic hiding of six weeks due to the unequal rotation periods of the earth and Mars, two striking canals were at once evident to the east of the Syrtis in places where no canals had ever previously been seen. Not only was their appearance unprecedented, but the canals themselves were the most conspicuous ones on that part of the disk. They ran one from the bottom of the Syrtis (lat. 20 deg. N., long. 285 deg.), the other from a point part way up its eastern side (lat. 17 deg. N., long. 284 deg.), and, curving slightly to the left as they proceeded south, converged to an oasis, itself new, on the Cocytus (lat. 5 deg. N., long. 265 deg.), about two-thirds of the distance to where that canal meets the Amenthes. The Amenthes itself was not visible, except possibly as a suspicion. With the two main canals were associated several smaller ones, and at least two oases which had never been seen before, and from the interconnection of all of them these clearly made part of the new piece of Martian triangulation.

The phenomena were recorded in many independent drawings by Mr. E. C. Slipher and the director, and in the course of the next few days were photographed, appearing on the plates to the eye as the most conspicuous canals in the presentment of the planet. It is opportune that detailed photography of Mars in Mr. Lampland's skillful hands should have been so perfected as to make this possible; for the photographs taken by both Mr. E. C. Slipher and the director record these canals so that anyone may see them. There are thirty images, more or less, on each plate, and the canals appear on every image; on some more distinctly than on others, owing to the state of our air at the time, but recognizably on all; for each image had a pose of about two seconds and a half, and its definition varied according to the seeing at the time. Owing to the grain of the plate being much coarser than that of the eye, the two canals appear merged in one in the photographic images as a single line, its linear character, however, being quite distinct to one of good eyesight.

The photographs of this region taken in 1907 show no such feature.

No remembrance of ever having seen them before could be recalled by either observer, both being familiar with the planet, except that Mr. Slipher turned out to have drawn one of them the evening previous.

The record books were then examined, when it appeared that not a trace of them was to be found in the drawings of August, July, June, or May when this part of the planet was depicted. That they had not been observed in previous years was then conclusively ascertained by examination of the records of those years. The record of canals seen here is registered after each opposition in a fresh map of the planet's surface. This has been done since the beginning of the critical study of Mars at this observatory in 1894. Now, when these maps came to be scrutinized for the canals, each of them failed to show any such features. Nor had any observer previous to 1894 recorded them, as the observatory library of the subject bore witness. Schiaparelli had never seen them, nor had his predecessors or successors. This determined definitely that no human eye had ever looked upon them before. But, stirring as it is to know that one is the first to see a new geographical feature on another planet, akin to the thrill of finding unknown land in our own Antarctic regions, a much deeper scientific interest attaches to the question whether a phenomenon previously undiscovered was also previously non-existent. For in that case one has seen something come into being, with all that such origination implies.

It might seem to persons not versed in the subject that its absence on the charts was proof that a canal was itself new in the second sense because it was so in the first; but study of Mars has shown that this cannot be taken off-hand for granted. Several points must each be carefully considered. In the first place, one must be sure that the phenomenon could have been seen before, yet was not. It must be of a size which could not have escaped detection previously. Now, the great majority of canals discovered here were beyond the hope of detection elsewhere, owing to the character of the air, the improvement in instrumental means, and the long acquired knowledge of the observers. That they were not seen by Schiaparelli, therefore, by no means implies that they did not exist in his day—or even in the earlier days of observation here. We see to-day vastly more than we did in 1894, because of the experience acquired since. In the present case, however, this possibility of error was excluded by the size of the canals in question. They were not difficult detail of the order here mentioned, but, as I have said, the most conspicuous on the disk, canals which no observer of any standing whatever in good air could possibly pass by. They would strike any skilled observer of such matters the moment he looked at the planet. So far as this point went, then, they could not have existed before.

The next point to be considered was whether they were results of a characteristic of the planet of vital import in its cartography—the annual seasonal change which affects all its features. For the world of Mars is as subject to recurrent seasonal change as our own, and more markedly so. This change stamps itself unmistakably upon all its features, obliterating some and bringing into prominence others according to the time of the Martian year. Examples of this occur in the study of Mars regularly at each opposition, the aspect of the disk varying according to a definite law dependent on the Martian season. To be sure, therefore, that a canal is itself new, the planet must have been previously carefully depicted at the same season of its year, and then when these earlier drawings are critically scanned the canal in question not found recorded on them. Now, the possibility of definite and conclusive intercomparison of the sort is not presented so frequently as one might think. Mars comes to opposition each time later and later by about two and a quarter of our own months. This means that we meet him in a different part of his orbit at each fresh approach, and so at a different season of his year. Now, until Schiaparelli's time, it was at or near opposition only that his face was studied. Schiaparelli extended the time greatly, but not until the subject was taken up at Flagstaff was the period of observation prolonged to six and eight months for each opposition epoch, thus enabling the same Martian season to be recurrently viewed by the seasonal overlapping of two or more observation periods.

But even so, the disk is not equally well presented in successive Martian years, because of the differing distances the two bodies are apart, and the difficulty of consequent comparison on the score of size. Still another difficulty in the way of parallelism is that of phase. Unless the two bodies be exactly opposite at the same season of the Martian year in the two cases, Mars will show a differing phase at each, and this means a different slant in the illumination. This is a very important distinction, because the disk shows very diversely when illuminated from above or from the side, so diversely that faintness of detail has often been attributed to intrinsic weakness of feature when illumination itself was the cause.

In consequence, the observer can never be quite sure that his data are comparable until he has himself seen the Martian disk under like conditions, or nearly such, which recurrent presentations demand a lapse of fifteen to seventeen years.

Furthermore, to be conclusive, the observations must all have been made by the same observer, working under like conditions, and grown, in consequence, familiar with every detail of the disk, since the personal equation, including by that term the site and the instrumental methods and equipment, is always a factor. A Martian cycle, that is, a round of about sixteen years, must have been gone through by the same observer before definite judgment can be pronounced. Such a cycle now stands complete at Flagstaff.

Examining the records we find that Mars was observed four times previously at the same season of the Martian year as occurred during the epoch of the appearance of these two canals. The canals were seen at this opposition as follows:

	1909	☉	In Martian Relative Chronology Northern Hemisphere	Helion Longitude of Planet	Opposition.
First appearance..	Sept. 30	277°	Jan. 6	4° 50′	Sept 28, 1909
Last Observed....	Dec 12	320°	Feb. 17.	47° 43′	

The previous occasions on which the canals should have been visible, if their appearance or non-appearance were a consequence of the Martian season, were:

	☉	Martian Dates.	Opposition.
1894 September 8	277°	January 6	October 20, 1894
to November 18	320°	February 17	
1896 July 25	277°	January 6	December 10, 1896
to August 23	320°	February 17	
1898 July 12	295°	February 9	January 18 1899
to August 23	320°	February 17	
1907 November 12	276.5°	January 6	July 5, 1907
to January 25	320°	February 17	

During all these periods the planet was kept under observation at Flagstaff, and during none of them were any such canals recorded. We are, therefore, sure that seasonal change cannot explain them, and that two years ago, and also eleven, thirteen, and fifteen years ago, no such canals existed. In Martian chronology this means that not only did they not exist in their present state during the previous Martian year, but also not four, five, and six Martian years before that. It is also fairly sure that they were not in existence thirty and thirty-two years ago, inasmuch as Schiaparelli never saw them.

Lastly, a further point disclosed by the Flagstaff observations must be reckoned with, a point of very singular significance. It was long ago discovered there that (see Bulletin No. 8 of the Lowell Observatory), while the great majority of the canals are quickened into conspicuousness alternately every six Martian months, first from the south and then from the north polar cap, certain ones respond only to one or the other cap, remaining inert to the action of its antipodal fellow. To be sure, therefore, that the new canals were really new to Mars, the old drawings had to be examined on this score too. Here again the records were decisive. No such canals had ever appeared before from the quickening of either cap at the time when, had they existed then, they should have showed.

The canals in question, therefore, proved to be not simply new canals to us, but new canals to Mars. In the canal system they are *novae* in fact or function, and as such are the most important contribution to our knowledge of the planet of recent years. For let us see what they imply. In form they are like all the other canals, narrow, regular lines of even width throughout, running with geometric precision from definite points to another point when an oasis is located. This oasis resembles all the other oases, a small, round, dark spot. They partake, therefore, of all the peculiar features of the canal system, features which I have elsewhere shown make it impossible of natural creation, that is, of being the result of any purely physical forces of which we have cognizance. On the other hand, the system exactly resembles what life there would evolve under the conditions we know to exist. The present phenomena, then, show that the canals are still in process of creation, that we have actually seen some formed under our very eyes.

Thus, on every point which had to be considered, the records furnished conclusive evidence that the canals in question could not have existed in past Martian years in the condition in which we observe them to-day. Their previous non-visibility could not

* Nature.

have been due to any of the causes which might possibly affect it, to-wit: (1) Want of size; (2) any personal equation of the observer; (3) improved instrumental or atmospheric means; (4) distance (all these are negatived by their striking conspicuousness); (5) phase; (6) regular seasonal change, and last (7) unihemispheric seasonal change.

It will be perceived that the proof that these canals are *novae* has been possible, and only possible, through the long systematic work done on the planet here for the last fifteen years. Without such a complete system of records the certainty that the canals in question were new canals to Mars could not have been reached.

Lowell Observatory, Flagstaff, A. T.

POLONIUM.

SOME NEWLY DISCOVERED PROPERTIES.

BY MDME. P. CURIE AND A. DEBIERNE.

It is known that among the new and strongly radio-active bodies polonium was the first to be discovered. Many efforts have been made already to isolate this substance and to characterize it as a chemical element, but in spite of the great activity of the product obtained this result has not yet been achieved. According to the theory of radio-active transformations the quantity* of polonium present in radio-active minerals must be very small. According to this theory polonium is looked upon as a descendant of radium, and the relative proportion of these substances in radio-active equilibriums is equal to the ratio of their mean lives. The mean life of radium being about 5,300 times greater than that of polonium, and radium being found in pitchblende in about the proportion of 0.2 gramme per ton, it is seen that the same mineral cannot contain more than about 0.04 milligramme of polonium per ton. Many problems of supreme importance in radio-activity are connected with the isolation of polonium. This body is an unstable element which apparently represents the last radio-active term in the series derived from radium; we may therefore hope to show the formation of an inactive element derived from polonium. Moreover, polonium giving rise to an emission of α rays should produce helium, and this production not yet having been observed, it is important to ascertain if there really is in this a fact incompatible with theory.

We have undertaken recently a chemical research with a view of preparing polonium in a concentrated state. This was performed on several tons of residues from the uranium mineral which were at our disposal for this purpose. The mineral was treated with warm strong hydrochloric acid, which has the effect of dissolving polonium almost completely. The solution, which contains no radium, was submitted in a factory to operations having for their object the extraction of its active matter. This treatment, which was done under our direction and which will be described in a more extended memoir, furnished about 200 grammes of about a substance having a mean activity about 3,500 times that of uranium, and which contained chiefly copper, bismuth, uranium, lead, and arsenic; its activity was due to polonium. We sought to purify this material by treatment in our laboratory.

For this purpose the hydrochloric solution was precipitated with ammonia, to remove copper, and the precipitated hydrates were boiled in a solution of soda to dissolve the lead; they were mixed next with a warm solution of ammonium carbonate to dissolve the uranium. All these operations were repeated several times. The insoluble carbonates finally obtained were dissolved in hydrochloric acid, and the solution was precipitated with stannous chloride. These operation together were very successful, the original activity being found in the final precipitate in a sufficiently complete manner, which we verified by appropriate weighings.

The precipitate, which weighed about 1 gramme, was redissolved, and the hydrochloric solution was precipitated by sulphureted hydrogen. The sulphides were washed with sodium sulphide, then redissolved, and the solution was reprecipitated with stannous chloride; the resulting precipitate weighed a few milligrammes. Spectrum analysis, effected on this material, showed the presence of a diversity of elements—mercury, silver, tin, gold, palladium, rhodium, platinum, lead, zinc, barium, calcium, and aluminium; some of these elements being derived from the vessels employed. On attempting to purify the active matter we encountered great difficulties, and we found it very difficult to obtain without loss a substance of a more simple composition. Thus, on trying to separate lead by treatment with potash we found that a large part of the polonium passed into solution, although we were able to utilize without danger the same reaction in the presence of elements insoluble under these conditions. From this alkaline solution, polonium can only be reprecipitated by the addition of an alkaline sulphide. The reactions which we always found to be trustworthy are: Precipitation as sulphide from an acid or alkaline solution, and precipitation with stannous chloride. We also have found that polonium is easily deposited by electrolysis, and this method may be utilized for a quantitative separation when we wish to extract polonium from an acid solution, but at the same time other metals, such as gold, platinum, mercury, etc., are deposited. After many experiments the activity was concentrated in about 2 milligrammes of matter.

The activity was measured by an electric method. For this purpose a known and very small portion of the solution was evaporated on a thin plate of glass, and the absolute value of the saturation current obtained with this plate was measured in an appropriate apparatus. Knowing the value of the charge of a gaseous ion (4.7×10^{-10} E. S. units) and the number of ions produced by an α particle of polonium along its complete path (about 1.6×10^{5}), we can calculate the number of α particles emitted per second. Knowing, moreover, the speed of decay of polonium (a reduction to one-half in 140 days) and the number of molecules contained in a molecule-gramme (about 6×10^{23}) we can calculate the amount of polonium present, its atomic weight being supposed to be near 200. We also can calculate the amount of helium which is formed in a given time, assuming that each α particle is an atom of helium.

In this way we found that the quantity of polonium obtained would be about 0.1 milligramme; this quantity is what ought to be found according to theory, in 2 tons of good pitchblende. Our active matter therefore might contain several per cents of polonium, so that spectrum analysis might be attempted with some chance of success. Many spark spectra were obtained and photographed; unfortunately each of these operations involved a considerable loss of material.

The appearance of the spectrum is complicated; many elements being present, gold, platinum, mercury, palladium, rhodium, and iridium. Some spectra also showed the presence of alkaline earthy metals, which came probably from the attack of the vessels employed; these were removed by electrolysis. After a careful examination of the different spectra and the identification as complete as possible of known lines by their wave lengths (records of Exner and Haschek, Watts), or by comparison spectra obtained with the same spectrograph, some lines were left which might be attributed reasonably to polonium. The wave lengths of these lines are:

Weak	4,642.0
Rather strong	4,170.5
Weak	3,913.6
Very weak	3,652.1
The following lines are of doubtful origin:	
Medium, may be a parasitic line	4,651.5
Medium, may be identical with the aluminium line 3,961.7	3,961.5
Weak, may be identical with the platinum line 3,668.6	3,668.5

The accuracy of these measurements is pretty good, the error probably not exceeding 0.3 Angström unit. For the identification of the lines we not only considered the wave length, but also the relative intensity.

We hope to examine the spectrum again when the polonium has disappeared; this will allow us to form a definite opinion on the attribution of the lines indicated above. We also may hope to see the spectrum of the element formed at the expense of polonium. According to theory this element should be lead; lead is not entirely absent from our product, but its spectrum is very faint.

We have observed that the active substance obtained does not give rise to induced radio-activity, nor to any appreciable emission of penetrating rays. We have observed also an extremely minute disengagement of radium emanation.

A portion of the solution was utilized for the study of the gas disengaged. The solution was introduced into a quartz tube, which itself was placed in an apparatus which could be completely freed from air. The solution disengages much gas; it is easy to observe the continuous formation of gas bubbles, proving the decomposition of water; this decomposition must be attributed to the action of the α rays of polonium. The gases disengaged are almost totally absorbed by the action of heated copper and oxide of copper, of potash, and phosphoric anhydride. The slight gaseous residue was collected and examined by one of us by the method formerly used for the examination of the gases disengaged by actinium and radium (Debierne, Comptes Rendus, 155, 1909). This residue is sensibly pure helium; its complete spectrum was observed, and the volume measured. The volume was equal to 1.3 cubic millimeter at atmospheric pressure, the accumulation having been going on for 100 days. This volume is very near that predicted by theory, which is 1.6 cubic millimeter. The fact of the production of helium from polonium is therefore established, with the predicted order of magnitude. We propose to make as accurate as possible a determination of this volume, together with experiments on the numeration of the α particles emitted, so as to obtain the value of the number of molecules contained in a molecule-gramme. This direct method seems particularly advantageous when using a solution of polonium, as in that case the α particles are completely absorbed by the liquid.

In the course of our experiments a curious effect of the rays was observed. The polonium was kept dry in a small quartz capsule. This capsule was found to be cracked in a large number of places under the substance. The production of these cracks may be attributed to electric discharges.

An abundant disengagement of ozone was noticed generally in the neighborhood of the substance.—Comptes Rendus, cl., pp. 386-389, February 14th, 1910.

A NEW APPLICATION OF ALTERNATING CURRENTS OF HIGH TENSION.

The Textile World Record describes a new process for removing the electrical charge which textile fibers and fabrics acquire in the operations of spinning and weaving. It is a well-known fact that wool, silk, and other textiles, under the influence of pressure and friction, acquire an electric charge, which they retain persistently. In wool-working establishments this electrification of the material causes several grave inconveniences. In spinning, the strands, having electrical charges of the same design, repel each other, so that, instead of lying parallel in the yarn, they protrude in loops. The electrification also causes adjacent strands to become entangled and torn. The individual fibers of the strands exhibit similar peculiarities. Instead of lying smoothly they separate widely, and give the yarn a rough and irregular character. The electricity developed in shearing the cloth is another source of annoyance. The clippings and dust acquire an electric charge opposite to that of the cloth, and consequently adhere to it. These troubles are most marked on bright winter mornings, when the dryness of the air favors the retention of the charge. The only remedy that has hitherto been found at all effective is the production of moist and hot air in the factory. The use of this expedient produces an almost insufferable temperature, and the result has become more and more unsatisfactory with the steady increase in the velocity of the machines.

Chapman, an American inventor, has devised an entirely new method of obviating all of these difficulties. The most important part of his apparatus is a steel tube about 1½ inch in diameter, which is slit longitudinally and incloses a well-insulated cable, which is traversed by an alternating electric current. The slit in the tube is filled by insulating porcelain buttons almost in contact with each other. The electric current ionizes the air around the tube to a distance of several inches, and the magnetic forces developed by the alternating current causes a continuous and rapid motion of positive and negative ions in directions at right angles to the tube. In the course of this movement, the fabrics come into contact with oppositely charged ions and thus lose their charge, while the ions which carry charges similar to that of the fabric are repelled. As an alternating current is used, both positive and negative charges are dissipated in this way. The process has already been used in America. It possesses an especial advantage from the hygienic point of view, as it not only avoids the heating and moistening of the air, but it actually disinfects the air by the production of ozone.—Umschau.

ENGINEERING NOTES.

Contrary to a widespread belief that hard woods give more heat in burning than soft varieties, the scientists at Washington are contending that the greatest heating power is possessed by the wood of the linden tree, which is very soft. Fir stands next to linden, and almost equal to it. Then comes pine, hardly inferior to fir and linden, while hard oak possesses 8 per cent less heating capacity than linden, and red beech 10 per cent less.—Domestic Engineering.

A novel method of catching fish was described in a recent issue of the Electrical Review and Western Electrician. A trolley line running between Franklin and Columbus, Ind., skirts the White River for a considerable distance, and it has been discovered that the trolley wire is frequently tapped to furnish current for fishing by electricity. An end of the wire is placed in the water, and the current stuns such fishes as come within its influence, so that they can be taken out with scoop nets.

Experiments made last year on the Swedish railways in regard to the possibility of employing peat as a fuel for locomotives of various types have not given the results which were hoped for. On the contrary, they show that the locomotives now in use are not suitable for the employment of peat, unless this is mixed with a large proportion of other fuel. In consequence of these results, the engineers of the Swedish government railways are engaged in designing locomotives better adapted for the combustion of peat, and with these locomotives the experiments will be continued.

In a paper read before the American Society for Testing Materials, Robert Job describes a simple test of the value of lubricating oils under service conditions. He found that when heated to a temperature of 450 deg. F. the oils which had given bad results showed a very marked darkening of color, while those which had proved satisfactory showed very little change. Another experimenter, E. Camerman, of the Belgian State Railway, has devised a test of oils for lubricating engine cylinders. By heating the oil to 400 deg. F. for six hours it is possible to determine the percentage of oil available for lubrication after allowing for that which escapes with the steam. This varies from a very low figure to as much as 60 per cent.

The distinguished gas engineer A. Witz states in Comptes Rendus that as the field for the application of oxygen has been largely extended, he urges the importance of using directly in the gas producer the hot exhaust gases from gas engines worked with suction producers. A column of coke could be maintained at a temperature of incandescence with little loss of the fuel, and the CO_2 and H_2O vapor would be decomposed by means of the sensible heat brought in by them. It would be necessary to add a little oxygen to the charge in the engine cylinder, but the nitrogen and CO_2 undecomposed would supply the quantity of diluent necessary to temper the force of explosion. Any back pressure on the engine due to resistance of the coke column in the producer throttling the exhaust, could be regulated by a valve in the exhaust pipe.

In a paper read before the Association Technique Maritime, A. Rateu describes some experiments which shed some light upon Lanchester's conclusion that the surface friction of a plate is 0.02 times as great as the pressure on the plate would be were it placed normal to the air current, whereas Zahm's and other experiments on surface friction show that the coefficient is about one-tenth of the above value. A partial reconciliation of the two views is afforded by Rateau's discovery that the resultant pressure is, in general, not normal to the plane, the variation differing with the angle of inclination. For planes inclined at 10 deg. to the air current the deviation of the resultant from the normal is 1 deg., which would correspond to a coefficient of 0.0017, instead of the 0.02 adopted by Lanchester. Rateau's treatment of this "fictitious" friction is based upon his theory of the screw propeller.

In an article published in Lumière Electrique, P. André describes the construction and trials of a new turbine introduced by A. Barbezat and constructed in the Bollinckx Works. The principle of construction is that of a combination of a De Laval wheel and a Parsons reaction rotor on the same shaft, the drum of the reaction portion being made conical in section, as suiting better than a horizontal cylinder form the escape of the steam from stage to stage with minimum loss of speed or amount of friction. A single balancing piston can also be used, and this is provided by the large end of the conical drum itself. The idea in this turbine is to unite the advantages of the De Laval turbine for high-pressure steam (with a reduced peripheral velocity of the wheel) and the reaction principle of Parsons turbine for the lower pressures, this being considered the most efficient portion of the Parsons machine. The trials show fairly good results, and it is expected that some improvements will be carried out in subsequent machines.

SCIENCE NOTES.

Scandium is one of the metals of the "rare earths" which occur in certain minerals. It appears to be pretty widely distributed, for Sir William Crookes, in his spectroscopic examination of 53 minerals, found scandium in 10 minerals: anerlite, cerite, keilhanite, mosandrite, orangite, orthite, pyrochlore, thorianite, thorite, and wiikite. The last-named mineral is the only one which contains much more than 1/100 per cent of scandium. The proportion of scandium contained in wiikite is 1.17 per cent. Wiikite is an amorphous black substance, about 5² times as heavy as water. It is not completely dissolved by strong mineral acids, but is readily decomposed by fusion with potassium bisulphite. Heated to redness in a quartz tube, it evolves water vapor, hydrogen, carbon dioxide, helium and a trace of neon.

How big is a meteor? W. H. Pickering estimates that a meteor of the third magnitude must vary from six or seven inches in diameter to a mere grain according as its brilliancy is similar to that of an electric arc or immeasurably greater. That view is combated by C. Fabri in the Astrophysical Journal. From actual measurement Fabri finds that a star of zero magnitude gives an illumination of about 2.1×10^{-6} candle-meters, when the star is at the zenith and the observer at sea level, under good atmospheric conditions. This would mean that a meteor of third magnitude, 150 kilometers from the observer, should have a luminous intensity of 3,000 candles, or, allowing for atmospheric absorption, say 4,500 candles. The crater of an electric arc gives about 200 candles per cubic millimeter. Such a meteor would have a diameter of about 25 millimeters, and if of density 4, would weigh about 1/3 gramme or 4.5 grains.

Is there any meteorological evidence of a change in the rotation of the earth's axis? F. von Kerner thinks that there is, and presents his views in the Meteorolog. Zeitschr. He publishes a table which gives for the whole year, and also for January and July, the difference in mean temperature between successive belts of 10 deg. latitude of the northern hemisphere. For comparison the greatest and least difference occurring between each pair of belts are also given. The variations of the differences actually occurring along a belt are found to be commensurate with the changes which may be assumed to be due to a shift of the pole, and hence we must be very cautious in drawing conclusions as to the probability of a shift of the pole from meteorological evidence. Thus in January for the belt 60 to 70 deg., the mean difference of temperature between its northern and southern limits is 9.5 deg. C., but in longitude 145 deg. W. (Alaska) it is as much as 25 deg. If we suppose a winter flora on the south coast of Alaska (lat. 60 deg.) becomes fossilized, and a shift of the pole through 10 deg. occurs along the 145th meridian, and if, further, the change be accompanied by a change in the configuration of land and sea, such that the January temperature in lat. 70 deg. is unaltered, we might find in lat. 70 deg. a fossil flora which would indicate a January temperature higher by 25 deg. From the mean distribution of temperature along this belt we might conclude that a shift toward the pole through 27 deg. of latitude had taken place, whereas the actual shift has been assumed to be only 10 deg. Similar arguments are brought forward for other parts of the world.

What is known as a white rainbow is an extremely rare phenomenon. It was observed at the Montsouris Observatory at Paris by M. Louis Besson on the 5th of February. It was an almost colorless bow and was seen at 2:10 P. M., dying out and then reappearing at 3:15, reaching a maximum brightness at 3:25, then disappearing five minutes later. The bow had about 3 degrees width and was not a pure white, but somewhat tinged with rose color at the outer edge and violet at the inner edge. At its brightest, it had an interior darkish band of somewhat violet hue. The angular height of the summit was 40 deg. 8. min. on the average. There have been often observed in the mountains or the polar regions white bows upon fogs or clouds composed of liquid drops. The explanation of this phenomenon, known as the "Ulloa circle," was given by Mascart. It is only a special case of the general theory of the rainbow as given by Airy, which allows of supposing a mixture of the colors so as to approach white, at the same time as a widening of the arc and a diminution of the radius when the diameter of the drops becomes smaller and comes near to 41 μ. But the present phenomenon appears under very different conditions from what accompany the Ulloa circle. The heavens were covered with an elevated cloud layer with small density (alto-stratus) and owing to the general conditions of the sky and the great height of the clouds we are led to suppose that the elements of the clouds which produce the rainbow are rather of an icy than a liquid nature. The author states that he knows of but a single case of an analogous phenomenon, which was seen in England on February 2nd, 1908, also in clear weather and with about the same kind of clouds.

TRADE NOTES AND FORMULÆ.

Wood Preservative Paint.—300 parts of well washed and sifted white sand, 40 parts of copper oxide, 1 part of sulphuric acid. The four first-named ingredients to be melted and mixed and then the copper oxide and sulphuric acid added.

Waterproof Paint for Wood.—The wood is painted with zinc-white, rubbed down in glue water (size); several hours later apply a coating of very dilute chloride of zinc. The resultant combination (zinc oxychloride) protects the wood from growths of mold, thickness and moss.

Badigeon is a paint mixed with slaked lime and stone dust or ocher, resembling stone mortar. Italian badigeon also known as normillo is a finish made from lime, Spanish white and a color addition, applied in layers and rubbed with hard brushes or woolen rags until it acquires a polish.

Fireproof Paint.—35 parts zinc white, 15 parts air-slaked lime, 25 parts white lead, 5 parts sulphate of zinc. Mix the first two ingredients and grind them, adding oil, then add 4½ parts of water glass (35 per cent), next the white lead and sulphate zinc, stirring all well together. This makes a white paint. If other colors are desired any coloring substance can be added.

Antacid Varnish for Leather.—Gall nuts 200 parts, logwood 30 parts, water 200 parts, is boiled for two hours, the water evaporated to be replaced from time to time and filtered. Dissolve in the filtrate 40 parts of green vitriol, 200 parts of brown syrup, boil the fluid again until it begins to thicken, then add a solution of 10 parts of ruby-shellac in 30 parts of strong alcohol. Must be kept in tightly closed bottles.

Asphalt Cement for Leather Belts.—Asphalt 12 parts, rosin 10 parts, gutta percha 40 parts, sulphide of carbon 150 parts, petroleum 60 parts. The materials, without the sulphide of carbon, are first treated in a bottle standing in boiling water, with the petroleum, the thick mass is allowed to cool; add the sulphide of carbon and allow the whole, frequently shaking it, to stand several days. The belts, evenly coated with the cement, should be exposed to heavy pressure between hot rolls.

Asphalt Lubricant for Wagon Wheels.—Asphalt 30 parts, black pitch 8 parts, litharge 8 parts, water 50 parts, petroleum 8 to 12 parts. The asphalt is melted in a kettle, equipped with a stirring apparatus, then the pitch and the litharge stirred in, 8 parts of petroleum being at the same time added. Finally the water is added in small quantities, the remaining 4 parts of petroleum being used, if added, to make the lubricant thinner. For use in winter it is made thinner than in summer.

Finish for Leather.—*a.* 125 parts of brown shellac to be dissolved in boiling solution of 26 parts of borax, and 3 parts of caustic soda, the powdered shellac being previously moistened with some ammonia. *b.* 56 parts of log wood extract solution, of 30 deg. Bé. (approximately 400 parts of extract to 1,000 parts water), 10 parts glycerine, 10 to 20 parts of aniline black and bethylene blue dissolved in 300 parts of water, mixed with the shellac solution. Add a mixture of 50 parts pyrolignite of iron of 20 deg. Bé. and 90 parts of water and add oil of mirane and thymol.

Sulphur Chips for Sulphuring Casks.—Melt stick sulphur in an enamelled iron vessel, heat it to 312 deg. to 362 deg. F. and draw through the molten sulphur pieces of unsized, fine writing paper 10 inches long and ¾ of an inch wide. The strips of paper, coated with sulphur (sulphur chips), are suspended, alight, on a wire hook, inside the bung hole in the cask to be sulphured and allowed to burn there. Continue burning the sulphur chips in the cask as long as they will burn; and then bring up the cask. Before it is filled again it must be washed out with water several times.

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